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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY



Edited by Glyn Daniel

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EDITORIAL NOTICES

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Antiquity

VOL. XXXII No. 127

SEPTEMBER 1958

Editorial

THERE must be few archaeologists at the present day who ever look at Boutell's *Manual of British Archaeology*, published exactly a hundred years ago; yet it, together with Akerman's *Archaeological Index* (1847) and Vaux's *Handbook to the Antiquities in the British Museum* (1851) are excellent examples of the methodological muddle which British prehistory was in before archaeologists boldly adopted the three-age system expounded philosophically and historically by Vedel-Simonsen, and used for the ordering of his museum in Copenhagen by Thomsen, and the study of Danish field antiquities by Worsaae. Worsaae, 'the first full-time professional archaeologist in history', as Bibby has recently described him, came to England in 1846, at the request of the Duke of Sutherland, whose brother, Lord Ellesmere, was busy translating into English the *Ledetraad til Nordisk Oldkyndighed*. He visited Ireland in 1846 after he had studied the antiquities of Britain, and said 'the same division of the antiquities into three classes—those belonging to the periods of Stone, Bronze and Iron—which has been adopted in the arrangement of the Danish primeval monuments, will apply to the British remains.'

Boutell and Vaux had no use for such an application, and Kemble was even more outspoken. In his address to the Royal Irish Academy in 1857 he said he was forced to regard a man like Worsaae, with his ideas of the three ages of prehistory, as likely 'to betray us into grave historical errors' leading 'into an historical *reductio ad absurdum*'. But Worsaae's ideas prevailed; Franks's *Guide to the Exhibition Rooms* of the British Museum (1871) explains half-apologetically that 'the remains of the inhabitants of the British island, previous to the Roman invasion . . . have, for convenience, been classed according to their materials and in the order corresponding to that of the supposed introduction of such materials into this country.'

This 'order . . . of supposed introduction' became *the system* of British archaeology, and any labels in provincial museums which still survive marked 'Ancient British' or 'Druidic' are as much collectors' pieces as the objects they attempt to describe. How does this order stand today, a hundred years after Boutell's *Manual*, after Pengelly had begun excavating at Brixham, or Colonel Schwab at La Tène—a hundred years after Prestwich had stopped at Abbeville on his way to Sicily and examined Boucher de Perthes's finds in the Somme gravels? In the first place the 'order' has become infinitely more complicated with a Mesolithic and Neolithic, and with subdivisions of the Bronze Age, and a division of the Iron Age into A, B, and C, first suggested by Professor Christopher Hawkes in the pages of this journal (ANTIQUITY, 1931). In the second place we have realized that some peoples who appear in the archaeological record in the British Isles as technically 'Neolithic' may well be the impoverished descendants of societies that were metal-using on the continent of Europe. Thirdly, some people, like the Passage Grave builders of Ireland and Brittany who were clearly, at least in part, contemporary with metal-using communities in other parts of the British Isles and France, seem not to have buried bronze or copper tools with their dead; and are therefore formally 'Neolithic.' What then do we

mean by 'Neolithic' any more?—early peasant villagers who preceded a society using metal, or people who had lost the arts of metal-working, or people who had sociological tabus against leaving metal in their surviving archaeological contexts? Does the word any longer bring any order into our study of the prehistoric past? What has happened to the system?

In his *Neolithic Cultures of the British Isles* (1954) Professor Stuart Piggott introduced the idea of *primary* and *secondary* Neolithic cultures; the primary culture being the peasant village communities of Windmill Hill, and the secondary cultures being the result of acculturation between societies practising the Neolithic arts and surviving Mesolithic hunting and fishing communities. (And, parenthetically, what are the Neolithic 'arts' now that pre-pottery Neolithic cultures have been found in Greece, Cyprus, Jericho and Baluchistan.) Piggott's idea is a useful one, and the idea of a Neolithic-Mesolithic acculturation seems the only way to explain the culture of the inhabitants of the Paris Basin in the first half of the 2nd millennium B.C., when Chassey Neolithic agriculturists and Mesolithic survivors together with megalithic chieftains (or traders or saints, according to how you feel in this matter) from the west produced the culture unfortunately labelled the Seine-Oise-Marne by Bosch-Gimpera and Serra Rafols some twenty-five years ago. But Piggott's idea of secondary Neolithic cultures is not what the French understand by this phrase, and those who are interested should read Professor Hatt's words on this subject in the *Bulletin de la Société Préhistorique Française* for 1956 (p. 435). Here he takes Déchelette to task for having included in his Bronze I what Hatt distinguishes as 'Bronze Ancien' and 'Chalcolithique', and regrets that Bailloud and Mieg de Boofzheim in their *Les Civilisations néolithiques de la France* (1955) make the same mistake. Hatt sees metal as having been introduced into France twice, once in his Chalcolithic, and secondly in his Early Bronze Age, and between them comes the 'Néolithique Tardif'—the Secondary Neolithic—or should it be called the Second Neolithic? And it is clear from correspondence with French colleagues that in some areas they would like to think of cultural periods in the following succession, Primary Neolithic, Chalcolithic, Secondary (or Late) Neolithic, and then the Early Bronze Age. What has happened? Simply this: the archaeological material is not now being thought of in terms of chronology at all: Kemble was wiser than he knew—we *are* being betrayed into grave historical errors, and are being led into an historical *reductio ad absurdum*.

We publish in this issue a short note by the Secretary of the Royal Commission on Ancient Monuments in Wales and Monmouthshire on the extension of Piggott's acculturation terminology into the Iron Age. Mr Hogg suggests to us (pp. 189–90) that we should begin thinking of the 'Secondary Iron Age' in Britain, and his basic ideas are most stimulating. But is the nomenclature right, or shall we, as we extend this valuable idea of impoverished and acculturated societies (implicit in Sir Cyril Fox's first analysis of the role of the Highland Zone in prehistory), get so caught up that we shall be talking of the builders of Tre'r Ceiri as Secondary Romans, and of the Celts who lived to the west of Offa's Dyke as Secondary Mercians? Where have we got to, anyway, when English and French colleagues cannot understand each other when they talk of the Secondary Neolithic? It seems to me that we have got to the end of the road which prehistory took when it left the Ancient Britons and Gauls and Goths and Druids of Boutell and Ackerman and Vaux and followed the lead of the Danish prehistorians. That was a necessary and right road, and without it there would have been no prehistory worth the name in the 19th century. But now we must take a new road, while recognizing gratefully that we could not have come this far without the three ages of Stone, Bronze and Iron. This is a new road along which we must distinguish at once whether we are speaking in terms of

taxonomy, culture or time, for as Robert Braidwood has so often emphasized in the last few years, we must have a trinomial system of archaeological nomenclature. But it is the nomenclature that is difficult to invent: what are we to call Piggott's Secondary Neolithic cultures and Hogg's Secondary Iron Age cultures if we want to retain these invaluable ideas and yet give them names unprejudiced by the old three-age system which is a thing of the past. It is nomenclature for cultural groupings that is most difficult; for chronology it looks as if, at long last, we can adopt a system of objective dates—and this is the great methodological revolution which Carbon 14 dating (discussed below, p. 193) will cause in archaeology. Already some of our more forthright and progressive colleagues are there; we now remember Boutell and Vaux. In fifty years time Atkinson's *Stonehenge* (1956) will be hailed, as we hail it here, as the first book which uncompromisingly refused to discuss that astonishing monument in any chronological terms other than those of calendar years. This is the way; this the road forward, and valuable though the historico-sociological ideas be that lie behind Piggott's Secondary Neolithic and Hatt's Néolithique Tardif and Hogg's Secondary Iron Age, they are themselves, to a certain extent to be classified as *le préhistoire du dix-neuvième siècle tardif*. Gabriel de Mortillet would have understood them, and that is the way back.

It may seem a long way in any direction from Boutell and de Mortillet and the *néolithique secondaire presque tardif* to Greek strikes and Irish fairies, but these have been a matter of antiquarian interest in the last few months. We have become accustomed in western Europe to strikes of bus and dock workers in England and even school teachers and university lecturers in France, but in Greece recently it was the archaeologists who struck. It is well known that the Greek archaeological service is in a bad way in the matter of personnel. The regular positions, i.e. the posts of *ephor* and *epimelete* are only about half filled so that, so rumour has it, there are only thirty men in the whole service for the whole of Greece. Each official is therefore responsible for an impossibly large territory. He has an infinite amount of running around to do, has to dig here, there and everywhere on an extremely limited budget and has little or no time left for scholarly pursuits. Nor are there adequate archaeological libraries apart from Athens, Herakleion and Thessaloniki. The chief reason for the depletion of the staff is, of course, the inadequate pay. We are told on very good authority that a senior *ephor* receives only about the equivalent of £450 to £500, and this inevitably means that such positions are tenable only, as a correspondent puts it 'by men with some independent means or by men who happen to have rich or industrious wives'. The guards of museums and sites are in an even worse state, being paid, when enlisted, at £7 a month and, after several years, reaching the maximum of £11 a month. After a great deal of discussion most of the scholarly employees in the Greek archaeological service recently went on strike; it lasted only three days and the government then granted a modest increase in salary. The guards then threatened to strike, but did not do so when their demands were immediately met, at least in some small measure. It is hoped that the new Greek Government will recognize the great role which their archaeological service plays, or ought to play in their national economy and prestige. After tobacco, tourism is the main industry of Greece. This industry will founder if its scholars and administrative staff are neglected and underpaid.

It is the fairies, not underpaid strikers, who have caused the trouble in Ireland. On 27 April, *The Times* reported that twenty-five workmen who had gone on strike for a fortnight because they refused to disturb a 'fairy fort' at Toorglass, near Belmullet, County Mayo, had gone back to work. They were employed by the Irish Land Commission and their job was to build a fence between two farms, but the fence led to and through a rath and the workmen stopped work because, as they said, interference with the fairies

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would bring bad luck and in some cases, early death. After a while a solution was found; the fence now goes round the rath and the fairies reign there undisturbed—at least in the minds of the workmen. The Dublin *Evening Herald* has taken umbrage at the British being amused at this incident, declaring that a belief in the fairies or 'the good people' abounds and survives especially 'in rural communities in countries whose roots go back to ancient times'. True, but the real question is, who are the good people in the study of the ancient past of Europe? They vary from time to time, and the fairies, with the Trojans and King Arthur and Samson, gave way to the historically attested Druids and Britons, as they were superseded by the users of stone, bronze and iron, and as they are now being superseded by cultural isolates from archaeological material analyzed taxonomically, geographically and chronologically. The question is, how secondary, how *tardif*, can the good people be?

The publication of the first report by the Leverhulme Trust of its Research Awards is a matter of reflection, encouragement and inspiration by archaeologists. The report (published by the Trustees at the Office of the Advisory Committee, St Bridget's House, Bridewell Place, London, E.C.4) describes first, in a lucid essay by Sir Hector Hetherington, the way in which the Trust's Funds were canalized to supplement to the point of effectiveness the resources of people who could not (through lack of money or pressure of routine duties) bring to completion some scholarly or scientific task on which they have been engaged'. Lord Leverhulme died in 1925: in May 1933 a statement was issued saying that 'the sum of £12,000 a year was devoted to a scheme for research fellowships . . . No subject of enquiry is excluded from the scope of the scheme, but, under present conditions, the Trustees intend that preferential consideration should be given to subjects in which existing provision is inadequate'.

Clearly archaeology was one of those branches of learning where provision for research was (and is) inadequate: and, looking at the report of the awards between 1933 and 1947, the Leverhulme Trust has realised this need and done well by archaeology. The names in this list include Prof. Garrod, Dr Leakey, Mr Creswell, the late J. L. R. Moir, Christopher Woodforde, Prof. J. M. C. Toynbee, Sir Mortimer Wheeler, Dr Philip Corder, L. P. Kirwan, J. R. B. Stewart, A. D. Lacaille, and Prof. Grahame Clark. It is pleasant to be able to record publicly the great help which the Trust has given and is giving to archaeological research. Elsewhere in this issue of *ANTIQUITY*, Prof. Coon refers with pleasure to the Carlsberg Trust and the generous support which that munificent foundation has always given to the sciences and humanities alike. It is true that, so far, we have nothing quite comparable to the Carlsberg Trust in the British Isles; there is no Bass, Worthington, Whitbread or Ind Coope Trust in Britain, no Guinness Trust in Ireland to support excavation, field survey and research. But we have gratefully, the Leverhulme Trust, and interested scholars should make a note of the address of the Trust.

Postscript. As we go to press, W. Creighton Gabel, who wrote in this journal last year on 'The Campignian Tradition and European Flint-mining' (*ANTIQUITY*, 1957, 90), has just published an article on the 'European Secondary Neolithic Cultures' in the *Journal of the Royal Anthropological Institute* (Vol. 88, 1958, 97), and this should be read in conjunction with a forthcoming note in the *Bulletin de la Société Préhistorique Française* by P.-R. Giot, J. L'Helgouach, and J. Briard, as well as M. Giot's comments in the first article in this issue.

The Chambered Barrow of Barnenez in Finistère

by P-R. GIOT

The two chambered long barrows at Barnenez near Morlaix were accidentally brought to light a few years ago by the pillage of a road-constructor. The writer of the present account, who is a Maître de Recherches au Centre National de la Recherche Scientifique in charge of the Research Laboratory of Prehistoric Archaeology and Physical Anthropology at the University of Rennes, and head of the Breton division of French prehistoric antiquities, was able to halt this destruction and excavate Barnenez himself, revealing it as one of the most remarkable monuments in prehistoric France, consisting of eleven Passage Graves in a row. His careful work also provided one of the first well attested examples of stratigraphy in a French megalithic monument. The results of this work, summarized here in English for the first time, have already been published briefly in Gallia (1956, 189; and see the forthcoming 1958 issue), the Bulletin de la Société Préhistorique Française (1956, 18 and 163 and 326; 1957, 358) Annales de Bretagne (1957, 9) and Bulletin de la Société Archéologique du Finistère (1955, 70; 1956, 52; and 1957, forthcoming).

THE estuary which runs north from Morlaix in the department of Finistère in Brittany is flanked on the right or east side by the peninsula of Kerneléhen; on the northern end of this peninsula near the farms of the hamlet of Barnenez (Plouezoc'h) are two big barrows or stone cairns. Both of these are long barrows orientated from east to west, the northern one being shorter than the southern. Both the barrows of Barnenez North and Barnenez South have been known to archaeologists for more than a hundred years, but have not been excavated until now. A century ago local people dug into what were described as underground passages, and the same was done by local amateur archaeologists during the last war, but they did not appreciate what kind of monument they were dealing with and did not report their work.

Four years ago a road contractor, attracted by the easy supply of road metal afforded by these big piles of stones, bought from one of the owners permission to demolish them. He began attacking Barnenez North in November 1954, and completely destroyed a Passage Grave which seems to have had a passage dry-walled and roofed with megalithic capstones, and a circular chamber similarly walled and roofed with a single large stone at the height of about 2 m.—the height of a man. On the site of the chamber the present writer and his associates discovered sherds of western Neolithic pottery. Part of Barnenez North, on the ground of another owner, is still intact. The whole barrow, which was oval, measured about 35 m. in length by 20 m. in breadth and 3 m. in height.

Later, in the spring of 1955, the same contractor began quarrying into the north-western end of the bigger barrow, Barnenez South, which is 85 m. long by 25 to 35 m. broad with a maximum height of 5 to 8 m. This huge artificial mountain was a good bargain for him. These new depredations resulted in the disclosure and partial destruction of four chamber tombs. But, at this moment, in spite of the fact that the contractor had not made the legal declaration to the authorities that French law requires in every case when antiquities are discovered, and in spite of the remoteness of the site, news of the robbing of Barnenez North got around; the present writer was informed *ex officio* and was able to set immediately in motion procedure which brought this illegal quarrying to an end.

Barnenez was then scheduled as a historical monument, and the State brought an action

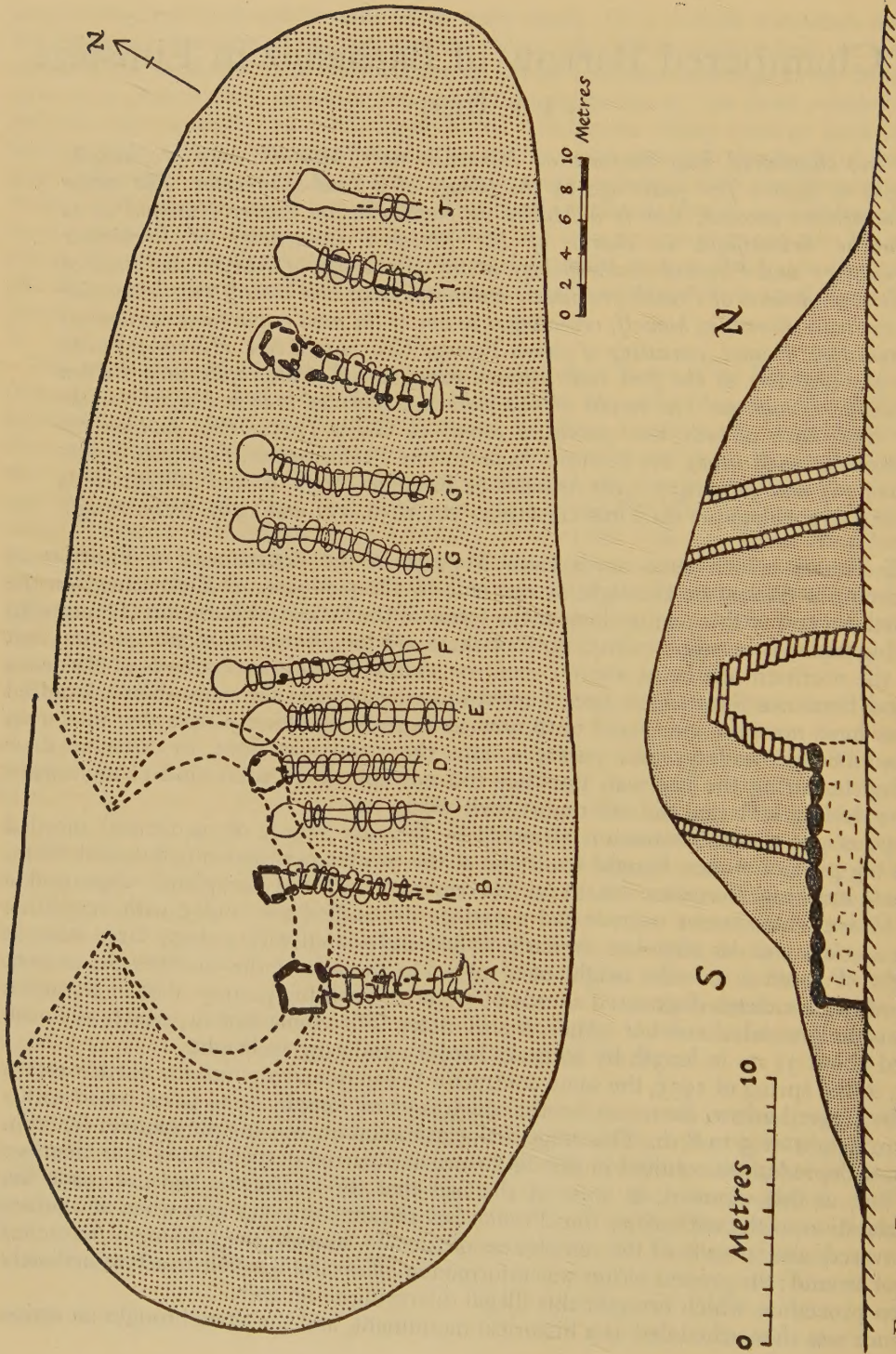


FIG. 1. Barnenez South (top) Plan of chambered cairn and (bottom) idealized section through one of the corbelled chambers.

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against the contractor which resulted in a sentence from the *Tribunal de Première Instance* at Morlaix, later increased by the Court of Appeal at Rennes. The contractor has not only been fined, but ordered to pay for the cost of the consolidation and restoration of the ruined burial chambers.¹ These sentences are of the very greatest interest and importance and create an excellent precedent for the application of the new French legislation on antiquities.

On the other hand, these unhappy events have, in the end, permitted the systematic excavation of the whole of the larger barrow, Barnenez South, with the help of grants from the *Direction Générale de l'Architecture du Ministère de l'Éducation Nationale*, with the result that the site is revealed as of the greatest importance in the study of megalithic architecture. The barrow comprises and covers no less than eleven Passage Graves, set side by side, opening on to the south side of the barrow (FIG. 1); these differ among themselves in their morphological and constructional details, and provide a unique collection of types. The present writer conducted the excavations at Barnenez in 1955, 1956 and 1957, with the assistance of J. L'Helgouach and J. Briard, both *Attaché de Recherches au Centre National de la Recherche Scientifique*. The propping up and strengthening of the chambers is being carried out under the direction of Monsieur R. Lisch, *Architecte en Chef des Monuments Historiques*. When at last accessible to the public, the burial chambers which have been cut open by the contractor will be without doubt more spectacular than the intact chambers, into which it will only be possible to penetrate by crawling along the passages. It is, of course, ironical that after a planned and systematic excavation one would never have dared to prepare such a spectacular and instructive exhibit as is provided by the contractor's slicing of the chambers (see PLATE XVII).

The barrow of Barnenez South is built of small stones kept in place by long revetting walls set obliquely and converging towards the centre of the mound. The stone used is locally obtained, and most of the material used in the construction of the barrow and the dry-walling comes from within a few hundred feet of the site. The volume of the mound is 6,500 cu. m., which in itself indicates the enormous amount of labour involved in its construction. On the other hand, all the big stones used in the megalithic construction of the chambers and passages are of a type of granite which comes from the island of Sterec, a mile off the end of Kerneléhen peninsula.

The rescue excavation consisted of exploring Chambers A, B, C and D; the systematic excavation comprised the passages leading into these chambers from the south, and then the discovery of the remaining tombs, and their careful exploration. The seven new Passage Graves discovered have been labelled E, F, G, G¹, H, I, and J. We will now describe these eleven Passage Graves briefly (FIG. 1).

A. This is the most westerly of the eleven Passage Graves. The passage starts with an entrance defined by orthostats; on the eastern orthostat of the entrance are seven engraved 'wave' symbols. Many of the orthostats walling the passage are tilted and the way in is very narrow. The chamber is built of a corbelled vault of dry-stones resting on enormous orthostats; the top of the corbelled vault is 4 m. above ground level. During the use of this tomb, and presumably to prevent some of the stones of the chamber and of the passage from tilting more than they do, large quantities of mud were brought in, and sherds of western Neolithic pottery were discovered under and on top of this layer of mud. The corbelled vault is now reduced to one-third by the depredations of the contractor's men.

B. This monument is entirely built of megaliths. The chamber is roofed with a single capstone. The orthostats of the passage, set in front of dry-walling, are at the present day much tilted inwards and it is very difficult to get through the passage. Sherds of western

¹ Cost fixed by expert's appraisalment at 1,440,388 francs.

Neolithic pottery and some fragments of human bones were found in the passage.

C. The walls of the chamber and the passage of this Passage Grave are entirely built of dry-walling. The passage is roofed with capstones, and has collapsed in the middle. The top of the corbelled vault of the chamber was 4 m. above ground level but has now been half destroyed. The finds from this chamber include sherds of western Neolithic pottery, bell beaker of local type, sherds of coarser ware, twelve transverse flint-arrowheads, a flint fabricator, and a western European tanged dagger of arsenical copper.

D. This passage grave is of much the same type as Chamber *C*, but the lower part of the chamber consists of a wall of interbonded orthostats and dry-walling. Most of the corbelled chamber is destroyed, but the passage is excellently well preserved, and yielded stratigraphical evidence. The lower level—Barnenez South D I—yielded sherds of western Neolithic pottery, some decorated with incised festoons, sherds of bell beakers of the best red ware with decorated concentric bands of oblique lines of incised dashes, a superb barbed and tanged flint arrowhead, four transverse arrowheads, a fabricator and fragments of burnt human bones. The upper level—Barnenez South D II—consisted of a mixture of stones and sherds of coarse ware in the style of the secondary Neolithic 'flower-pot' style, with everted bases and finger-tip decoration on the rim or on applied lugs. Together with this coarse pottery were found three polished greenstone axes, a fabricator, and an unpolished flint axe.

The chamber tombs designated *E*, *F*, *G*, and *G*¹ are of the same general style as *C*, but occasionally have orthostats walling parts of the passages. *E* has walls in perfect condition, but the top of the corbelled vault has collapsed. One sherd of uncertain ware was found in this tomb, which also yielded a few burnt oyster and limpet shells and a layer of burnt vegetable material, including grains of cereals, peas, bilberries and hazel-nuts. A radio-carbon date, kindly provided by Professor de Vries of Groningen, has shown that this vegetable layer is a medieval intrusion of only 750 years ago.

The passage leading to Chamber *F* ended in a collapsed structure so that we had to enter this monument, a perfectly preserved corbelled vault, through the roof itself, which was 4 m. in height. No finds were made in this monument. The passage of Chamber *G* was filled up half-way with stones, among which filling was a human tibia. The chamber itself was thus completely sealed: it was a perfect corbelled vault 2 m. 50 cm. in height. No funerary material of any kind was found inside this tomb in spite of the care apparently taken in sealing off the chamber against intrusion. This surely proves that either nothing was put in the tomb or that what was put there was of a perishable character. Chamber *G*¹ is very similar to *G*; the passage is again blocked half way, and the chamber itself, the same size as Chamber *G*, is perfectly preserved, and empty.

H is a complicated monument. The passage is entirely megalithic in architecture; the chamber is very large, walled by big orthostats, and is separated by two septal slabs into an antechamber covered by a corbelled vault (the top of which has fallen in), and a main chamber covered by a huge capstone. One of the septal orthostats separating the antechamber and chamber, and four of the orthostats of the main chamber are engraved with 'wave' symbols and other signs, including triangular axes, hafted axes, and a bow. A few sherds of western Neolithic pottery were found in this remarkable monument; these were extremely small and are the sum total of the finds.

I has a passage which is half-megalithically and half-dry stone walled. The chamber is a dry-walled corbelled vault of which the top is collapsed. In the filling of the chamber were Iron Age sherds and horse bones. *J* has a short passage which is badly ruined. It is roofed with capstones and on the underside of the first capstone there is an engraved representation of a motif classic in the megalithic art of southern Brittany—a stylized

THE CHAMBERED BARROW OF BARNENEZ IN FINISTÈRE

human figure of quadrangular form out of which splay, at one end, lines or 'hairs'. The collapsed corbelled chamber yielded Iron Age pottery.

Let us now turn from the description of these eleven Passage Graves to an interpretation of the monument as a whole. These eleven monuments are all contemporary, or very nearly so. *C*, *D*, *E* and *F* were built together from tangent bases: *C* was necessarily built after the building of the megalithic chamber *B*, on which its base rests. The primary grave goods common to six of the chamber tombs (though often there are very little) consist of western Neolithic round-bottomed undecorated vessels. Only two of the tombs, namely *C* and *D*, appear to have had a continuing use into Chalcolithic and Secondary Neolithic times.

Corbelled vaulted Passage Graves (referred to by some archaeologists as *tholoi*) are known here and there in Brittany, but are perhaps scarcer than capstone-roofed Passage Graves because they are more easily destroyed than megalithic monuments *sensu stricto*. Examples of such monuments in Brittany are Île Longue at Larmor-Baden, Morbihan, Île Carn at Ploudalmezeau, Finistère, excavated by the present writer in 1953,² Roc'h Avel and Guennoc at Landeda, Finistère, and the famous Yvias in Côtes-du-Nord excavated and planned by Martin in 1900.³ At Barnenez the intimate association of strictly megalithic architecture with corbelled vaults shows that both these technically different monuments are simply differing solutions to the same problem, solutions perhaps adopted according to the availability of material. The symbols engraved on some of the orthostats can be paralleled at once in the classic decorated tombs of the Morbihan.

On the whole, everything at Barnenez can be paralleled in Brittany, except the double chamber *H*, and this has no parallel at all in the whole of European megalithic architecture. But while every detail—or almost every detail—can be paralleled in Brittany, there is no parallel for the association of eleven Passage Graves in one barrow. The most that Brittany can show hitherto is five or six burial chambers in one mound, namely La Motte-Sainte-Marie, Pornic (Loire-Atlantique). Of course, in Normandy, the chambered barrow of Fontenay le Marmion (Calvados) contains the remains of a dozen corbelled vaulted chamber tombs. Barnenez South has thus given us a most interesting contribution to our knowledge of the variety of megalithic architecture. What is also interesting and sobering to note is that there is, to the best of our knowledge, no other unexcavated barrow of such proportions in Brittany, so that the opportunity afforded to the present writer and his associates in the last few years may never repeat itself.

² *Bulletin de la Société Archéologique du Finistère* (1955, 53); *Gallia* (1954, 404).

³ *Mémoires de la Société d'émulation des Côtes-du-Nord* (1900, 24).

The Hill-Forts of Northern France

by CHRISTOPHER HAWKES

In this article the Professor of European Archaeology in the University of Oxford reviews The Hill-Forts of Northern France, by Sir Mortimer Wheeler and Katherine M. Richardson (with an appendix on Muri Gallici by M. Aylwin Cotton) which is published as Report XIX of the Research Committee of the Society of Antiquaries of London (Oxford University Press, 1957, pp. xvi, 230 with 35 text figures, 50 plates: 50/-), and considers its bearing on the Maiden Castle report, originally published in 1943 as Research Report XII of the Society of Antiquaries.

THIS is the long-awaited Report on the expedition led by Sir Mortimer Wheeler (as he now is) to northern France, with Miss Richardson as second-in-command, in the summers of 1938 and 1939. The 1939 season was cut short by the outbreak of war, and the total time spent in the field (both seasons put together) was some thirteen weeks. The amount of work accomplished, in that time, is remarkable; anyone not knowing Wheeler's powers of organization and leadership will call it astonishing. Excavations were made in five hill-forts of the Gaulish Iron Age in Brittany and Normandy, and their results form naturally the core of the Report. But much more was done than this. While the excavations were going on, normally under Wheeler's personal direction, motorized teams of field investigators toured the whole of Brittany, Normandy, Picardy and part of Artois, covering 14 French Departments and part of a 15th, surveying every one of the earthworks in a list of some 200, drawn up in advance from published literature, and assembling from among them an inventory of 93 hill-forts and promontory-forts of undoubted Iron Age character. The resulting Gazetteer, with map, some sketch-plans, and a selected dozen out of who knows how many photographs taken, occupies pp. 102-132 of the Report, and is followed by an analytic and critical Bibliography of 155 items, ranging all the literature of these earthworks and of finds of the period throughout the territory.

The literature provided some information, but information of very uneven quality: sometimes, for example, Gaulish and Medieval works were not distinguished reliably from one another. Most of it consists of articles in French periodicals, more often regional than national, and hardly anywhere all accessible together. Lastly, the material had never before been put all under the scrutiny of a single mind—or certainly not of a mind like Wheeler's, at once archaeological and military. And out of all the 93, less than half a dozen had ever undergone excavation that could be called systematic, even by the standards prevailing in the 100 years before 1926, after which date, and until 1938, the Abbé Philippe's at the Fort-Harrouard (Eure-et-Loir) were the only such excavations done throughout northern France. The first thing that a reviewer must declare and proclaim, therefore, is that Wheeler and his team have made a magnificent and entirely fraternal gift to the archaeology of the great French nation. For our neighbours' future field-exploration and material-assessment of their northern Gaulish Iron Age, more especially (as will soon be seen) in its final phase, before and during the campaigns of conquest by Julius Caesar in 58-51 B.C., he has provided a firm and modern-style foundation. No one else, or at all events no one in the world of twenty years ago, could have done this in just this way.

Nor would anyone else—this is characteristically Wheeler—have built on the foundation simultaneously with laying it. He did this by recognizing at the start, with the help of

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preliminary surveys by Mrs Murray-Threipland and himself and Mr Raleigh Radford in 1935 and 1936-7, that in Brittany—lying opposite the great hill-fort and promontory-fort or cliff-castle regions of south-western England—two distinctive categories of Gaulish fortification stood out from the rest; by following this with the recognition of a further distinctive category in Normandy and elsewhere north of the Seine in Belgic Gaul; and by putting selected examples of all three categories, in 1938-9, to the test of excavation. The first category is the large-sized tribal *oppidum*, where it has not the character distinguishing the third. There are seven of these *oppida* in the coastal Departments between the lower Seine and the Atlantic, and at two of them Wheeler dug. The Report's account of the work begins with Le Camp d'Artus at Huelgoat (Finistère), of 75 acres internally and of two Gaulish phases structurally, the second adding a pear-shaped smaller fortified enclosure in the west end of the whole. It was presumably the *oppidum* of the Osismii, who were occupying the Finistère region in the time of Caesar. The other of these *oppida* excavated is Le Châtellier at Le Petit Celland, near Avranches (Manche), in south-west Normandy and probably the *oppidum* of the Ambibarii, the southern neighbours of the Venelli of the Cotentin peninsula. This fort gave Wheeler the clearest of all his evidences for date. Though little more than half the size of Le Camp d'Artus—42 acres internally—it resembles it in being much the largest Gaulish fortification in its district; the country round is in both cases poor, so that the large size must be understood in political or military terms, rather than terms of any rich regional economy. Its defences, both in the outer of their two lines and at the entrance, showed him that they were never completely finished. His excavations there and inside showed that the place was not inhabited for long. The entrance showed a single layer of occupation and construction; and this yielded 19 Gaulish silver coins, reliably dated—as the numismatist Dr J. Colbert de Beaulieu has confirmed—to the last phase of the Gauls' independence. (They correspond closely with those in the great La Marquanderie hoard found in 1935 in Jersey). These coins must, thus, have been circulating in the time of Caesar's campaigns; and, finally, as the excavations clearly demonstrated, the entrance-gateway had been violently destroyed. Wheeler and Miss Richardson infer that the *oppidum* was only built for the tribe's resistance to Caesar, and that before it was quite finished it was captured and put out of use by the army of Caesar's officer Sabinus, whom his chief records to have quelled hereabouts, in 56 B.C., the revolt of that year in which all the north-western Gaulish tribes joined the Veneti of Morbihan against him—those of the Manche and adjacent districts led by the Venelli, under their king Viridovix (*De Bello Gallico*, iii, 17-18). The inference is essentially a sound one. Defences still unfinished in 56 B.C. are unlikely to have been begun much earlier; the destruction is dated to that year pretty plainly; and the occupation-material, which included pottery as well as the coins, may with full safety be taken as typical of the district in that and the closely preceding years. A still less completed fort, intended for some 50 acres, is noted near Landéan in Ille-et-Vilaine (pp. 113-14), and seems to mark a similar undertaking attempted by the Redones (the known tribe of that district), interrupted by Sabinus before it had got so far. At Le Camp d'Artus the works had been completed, but there was again essentially only one phase of occupation, with pottery and on one site (inside) also a Gaulish coin of shortly pre-Caesarian date; and again, the gateway excavated (on the south-east) had been violently destroyed, soon—the excavators judged almost immediately—after its construction. The Osismii of Finistère revolted in 56 B.C. along with the Veneti and the rest, and the inference here presented, that this *oppidum* had a like history to that given at Le Petit Celland, is entirely reasonable. There remains the second structural phase, that of the pear-shaped reduced enclosure; rather conveniently, it is recorded that the tribes of this region prepared to revolt again in 51 B.C. (*B.G.* viii, 31),

and an analogous secondary work is to be seen in the large fort at Guégon, farther east (near Josselin) and perhaps the *oppidum* of the Curiosolites, another of these tribes. The revolt was still-born, but these works can well have been among the preparations that were made for it. Thus, even with this addition, both the excavated *oppida* can be assigned with good confidence to Caesarian times, and their material culture be accepted as characteristic, in these parts, of the second quarter of the 1st century B.C.

The outstanding feature of the excavation of both was the recovery of the main ramparts' internal structure. This represents in both the famous mode of Gaulish stone-and-timber building described by Caesar (*B.G.* vii, 23) as the typical *muris Gallicus* or 'Gallic wall': the timbers long and laid in courses horizontally across the line of the wall at narrow intervals, made fast within by clamping (Caesar does not say just how, but the numerous excavated cases show it was by nailing them to cross-brace timbers with stout iron nails, which are thus the critical surviving feature), and the stones and rubble laid between them and above each course till the full desired height was reached, the facing-wall, in front and sometimes behind as well, being of large stone blocks, with the rows of flush beam-ends showing in comely regularity along it, like the closed gun-ports of a Nelsonian battleship. At Le Camp d'Artus, for the first time, the excavators ascertained the original full height—12 ft., or 3·66 m.: it was defined, at the west end of the fort, by the material added above it when the second-phase enclosure was made. And now, turning on from the account of the excavations to pp. 159–225, we find Mrs Cotton contributing a full and solid monograph on all the certain and possible examples of the *muris Gallicus* type of fort, which are at present listed in the archaeological literature of France (19 certain, 7 probable), Belgium (1 possible), Germany (3 certain), Switzerland (2 reasonably certain), Yugoslavia (1 possible) and—a well-known but still mysteriously isolated addition to the list—in Scotland (the Burghead fort in Moray). Her indefatigable energy in visiting nearly all the sites, and in condensing the literature on them so thoroughly—her catalogue is followed by exhaustive notes on each of the certain examples and on all that has been dug from and written about them till 1954—has made this a very valuable factual contribution to the book. It must be read with her recent study of the Iron-Age use of timber-lacing in British fortifications (*Arch. Journ.*, cxi (1954), 26–105). And the readers of this will know already that the term *muris Gallicus* must be applied only to the walls with their timbers made fast with nails, which in effect form a special and elaborated category within the range of timber-laced construction in the Iron-Age fortifications of Europe generally. Timber-lacing in general was practised throughout the Iron Age, and before its last phase often had vertical as well as horizontal timbers; but the *muris Gallicus* proper was a specialization of it, which represents the culminating refinement of the Celtic wall-builder's art. She therefore naturally discusses the question of the date of its invention and flourishing; an advance summary of her conclusions was put by Wheeler into his contribution to the Clapham Memorial volume (*Arch. Journ.*, cvii, Supplement) of 1951. Both follow the earlier-received French view that as *muris Gallicus* forts are absent from the first Roman Province in southern Gaul, conquered in 123–121 B.C., they cannot be appreciably earlier than that date. The La Tène III culture acceptably associated with most of those that have been excavated, and the Caesarian dating on Wheeler's evidence for his two north-west Gaulish examples, just considered, show that this construction was indeed, as Caesar says, normal (though not universal) among the Gauls when his campaigns began in 58 B.C.

Yet (as Mr C. E. Stevens has lately remarked to me) one cannot be quite sure that none are even a little earlier than 121, for the construction could well have been devised in the Arvernian empire of Bituitus, with its heart in the *massif central* of France, which it was

a chief object of the Roman campaigns of 123-121 to break. It is indeed very likely to have been designed against the Roman combination of assault by fire and by battering-ram, which Caesar testifies to its efficacy in withstanding; but this does not mean it was first designed against Caesar himself. The first ruler in inland Gaul whom the Romans assailed was Bituitus. And if the idea did not occur first to his engineers before his downfall, it should in any case have been disseminated among the Gauls close after and in consequence of his defeat, when, and especially after the Romans defeated also the Cimbric-Teutonic invaders from the north some twenty years later, the wisest Gaulish rulers might be expected to take to acting on the notion that they might all be assailed by Rome one day. That some waited till the last moment, when Caesar did assail them, is compellingly clear on the Wheeler-Richardson evidence. But this is not to say that all did so: an invention so enormously laborious to carry out is not disseminated far and wide between the Danube and the Atlantic first in the thick of war, but while men have plenty of time to think and plan. When war comes, those who have not done that can indeed adopt it quickly, but only because the 'know-how' has already been matured; it is this that the Wheeler-Richardson examples show, and very well they show it. So long as that is kept clear, this account of the *muri Gallici* is as valuable historically as it is methodical and thorough in presentation. (But on p. 212 Hunsrück-Eifel II must precede the late La Tène period, when the walls of the Ring of Otzenhausen were built; and on p. 216, last line, the first word should be 'neighbourhood'). The question when late La Tène culture, La Tène III, superseded La Tène II in any given region, is notoriously at present a difficult one; there is no certain evidence for it before c. 90 B.C., and there have been several claims for still further retardation. The evidence for the start of the process ought to come from Celtic Gaul, and it has not yet been produced. Meanwhile, the biggest *muris Gallicus* in Germany is at Manching, where the excavations resumed in 1955 have confirmed that the material is of La Tène II as well as III (*Germania*, 35, 32-44, W. Krämer); Dr Krämer's further publication should be keenly watched for. Incidentally, one of the strongest arguments for a fairly early La Tène III anyhow in Belgic Gaul—not always appreciated on the Continent—comes from the fact that the daughter culture of the Belgae in south-east Britain is already La Tène III, though arriving (on Caesar's evidence) appreciably before his time, or c. 75 B.C. at latest. And with this thought, we must revert to the Wheeler-Richardson excavations, which in 1939 were carried on to Belgic territory near the lower Seine.

The two forts excavated here were Le Camp du Canada, two miles in from the Channel shore at Fécamp, and Le Châtellier at Duclair, overlooking the Seine between Le Havre and Rouen. Both are on promontories, and both are on the chalk of the Pays de Caux, so named from the Caleti who inhabited most of it in Caesar's time, and whose largest *oppidum* seems to be that at Sandouville near Le Havre, of more than 200 acres. Up the Seine inland, however, and probably including the Duclair fort, ran the lands of the Veliocasses, their chief *oppidum* being the immense one at Vernon, upstream from Rouen. Thus the two Wheeler-Richardson forts were capitals not of whole tribes but of sub-tribes within them; however, proliferation of forts seems characteristic of many of the Belgic peoples, and both these, while not maximal, are anyhow major strongholds. Le Camp du Canada contains rather more than 50 acres, and its rampart runs round all four of its promontory's sides; it is strongest across the neck on the south, rising 37 ft. above its ditch's present bottom. The excavations here showed that it was of massive but simple 'dump' construction, and that the ditch was steep on the outer side, but very wide and shallow. Exactly the same thing was found at the Duclair fort. Promontory sites, 'dump'-built ramparts with inturned entrances, and most of all this wide and shallow sort of ditch,

in fact make up the distinctive character of the third of Wheeler's three main categories of north French fort. It is here christened the 'Fécamp type', and the sixteen examples which the Gazetteer has recognized are all in the west of Belgic Gaul, from the Aisne to the lower Seine and Somme. The best known from earlier work is the Oppidum de Pommiers, near Soissons (Aisne), in the territory of the Suessiones and most likely the Noviodunum which Caesar attacked in 57 B.C. (*B.G.*, ii, 12), but found he could not storm because its rampart was so high and its ditch so very wide. Here again, it seems, is a specialization of Gaulish defence-work devised with Roman siege-craft in mind: to fill steep-edged ditches over 50 and often approaching 100 ft. in width with an *agger* for mounting an assault would be a biggish undertaking, and the Belgae appear to have preferred large 'dump'-built ramparts to the *muris Gallicis*, since much of their country had no rock to make its necessary stones, whereas in gravel or chalk a rampart could be made formidable by size alone. However, here too, construction which served against Caesar is likely to have been devised first a while before, though the date of any example, of course, is a matter for excavations in each case to show.

Neither in the Fécamp nor the Duclair case did these excavations prove the main works to have been erected actually against Caesar, though at Duclair they came quite near it. At the Fécamp fort, one can consider first the south-eastern entrance. This was not strictly original, in that its causeway was an artificial one over a ditch already dug right across; moreover, the long inturns of the rampart flanking the entrance-passage, though declared contemporary with its main line (p. 63), were given no cutting (pl. xxx) to show whether they were really of one build with it or not. And when on p. 65 the authors argue from the little-worn state of the passage surface that the life of the fort was short, but yet also that it was not a permanently-occupied *oppidum* at all, we can see that the Caesarian datings arrived at in 1938 for the Petit Celland and Artus forts had created something of a 'wish' for like results in 1939. However, of the three phases that they recognized in the occupation, they discerned indeed very little time between the second and third, which in their pottery run later than Caesar's conquest, but admitted perhaps fifty years between the first and second. It has to be said that the stratification on sites G, H, and L, which is stated to show this, is nowhere given us, nor are these sites indicated anywhere on the plan (pl. xxviii). But the first phase is subsequent, though apparently directly so, to the building of the rampart; and of the three potsherds illustrated from it, one is a rim that could be Belgic but does not declare itself of Caesarian age, and the others, of a 'corky brown ware' nowhere else here reported, are a plain base and a rim which in England would surely at first be taken for Iron Age A or B. Actually, all are stated to be certainly or probably wheel-turned; but could not the fifty years estimated between this and the second phase be the fifty years prior to Caesar? That would make the site not less interesting, but more.

At Duclair, the finds were more abundant. The occupation-layer overlying the tail of the main rampart produced a La Tène III brooch and pottery which included sherds of imported amphorae, and of some La Tène III wheel-made Belgic pedestals. But much of the rest of it (fig. 21) is admittedly not yet closely paralleled, and though nearly all wheel-made does not look so very late. Some of it, too, was found in the body of the main rampart, pointing to a prior occupation, which (the excavators suggest) might have been defended by the small bank and ditch across the promontory's neck outside the main work. The further works at the other end could not be dated, and it was here at Duclair that the excavations were cut short by the war. The conclusion (p. 79) that the main occupation belongs to 'the eve of' Caesar's conquest is, of course, all right if the length of an 'eve' may be left a bit elastic, but the fact of a prior occupation is clear, and this too could surely date from fifty years before Caesar, and should be all the more interesting, one day,

on that account. The gritted orange ware of nearly all the pottery, the finger-printed rim no. 21 and the simpler among the other profiles in fig. 21, suggest something earlier than La Tène III Belgic quite close in the background. That would be quite as welcome a comparison for the British Iron Age before the 1st century B.C., as the La Tène III Belgic is in this century itself. Nobody before has got even as near to that 'something' as this, and it is really exciting to feel from these indications that it must be really there. And of course the authors sense this too: see what they say of that great fort the Cité de Limes, on the white cliffs of Bracquemont near Dieppe: 'the most obvious challenge to English eyes . . . full of potentiality, certain to repay a properly equipped expedition with cross-Channel problems in mind' (p. 11). What is this potentiality? Turn to the Gazetteer, pp. 123-5, with the sketch plan fig. 34; is there, or is there not, a small earlier fort inside the huge main 'Fécamp' category *oppidum*, in its eastern corner and partly eroded by the sea? 'It seems possible', we read; yes, it does, and what the authors have done with their eyes turned first, and how naturally, to the recorded and dated and dramatic wars of Caesar, can surely be repeated—with the priceless advantage of being able to start from this Gazetteer—with an eye to spare for earlier times.

The subject of pottery points one next to pp. 84-100, where, under the title of 'Pottery Scrap-Book', a selection is illustrated and described of the drawings of Iron-Age pottery which exists—or existed in 1938-9—in the museums of northern France, or rather (anyhow as represented here) of north-western France, for the selection comes all from between the Seine and the Atlantic, and practically all, except four groups of La Tène III from Fort-Harrouard (fig. 25), from Brittany. It makes of course a very welcome start, and I hope we may have everything in the note-books of the expedition, before long, similarly drawn out and published. But first there is the second category of the forts to be considered, which I have deliberately left till last.

This second category consists of the coastal promontory-forts of southern and western Brittany, and the related forts just inland: 33 of them listed and mapped in all (p. 5). These cliff-castles correspond wonderfully to the description of them given by Caesar (*B.G.*, iii, 12). One of the category, just inland, was excavated in 1938 for the expedition by Mrs Murray-Threipland, and is published here: Kercaradec, at Penhars near Quimper (Finistère), a $5\frac{1}{2}$ -acre fort near the Odet estuary, with the multiple ramparts by which 10 of the 33 are distinguished, inevitably suggesting links with the multiple-ramparted forts of the south-west and beyond in Britain. At three more of them, all true cliff-castles—Kervedan on the Île de Groix, Vieux-Passage near Plouhinec, and the Vieux-Château at Sauzon on Belle-Île-en-Mer (all Morbihan)—supplementary excavations were done in 1939 by Mrs Murray-Threipland and published separately, in *Arch. Journ.*, c, (1943), 128-49. Of these, all with multiple ramparts, the last-named seemed, on the strength of her pottery, to go back perhaps to the later 2nd century B.C.; the first-named might possibly be equally early in the initial period of its structure, though its third (with large outer rampart and ditch) came perhaps well on into the 1st century.

At Vieux-Passage the pottery was like that of Artus and Le Petit Celland, so the occupation ran to Caesar's time more certainly. For Kercaradec, again from pottery, the dating here claimed is similar: to Caesar from after 100 B.C. The main rampart (innermost in a complement of three) was of rubble with an outer facing-wall sheer, and an inner one stepped, just as at Gurnard's Head in Cornwall (*Arch. Journ.*, xcvi, 96), where Miss Gordon's wheel-turned pottery must indicate Breton connexions likewise. That the multiple ramparts (and the Kercaradec stepped rampart) indicate sling warfare, and that the like ramparts in Britain do so most powerfully, was one of the major demonstrations of Wheeler's renowned work of 1934-7 at Maiden Castle in Dorset (no. XII of these

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Reports, published 1943). And since it was from the Maiden Castle findings that the French expedition sprang, a reviewer is really bound to turn back to those findings when he has the French ones at last before him. The interval of fifteen years, so far from meriting any grumble, is a positive advantage; the emotions aroused by Maiden Castle were rightly big ones, and it is their due, and the due of the great years of digging that engendered them, to be recollected in tranquillity.

Multiple ramparts as the product of sling warfare are of course universal doctrine now, and will remain so. But when was this development, of the use of the sling and the multiplication of ramparts as defence against it, brought to our country? and where was it first received? The Maiden Castle thesis was that it was brought first to Cornwall, well before the time of Caesar, perhaps anyhow no later than the 2nd century B.C., in consequence of the trading-connexion with that land of mining, and above all of tin, maintained as Caesar tells us by the Veneti, who are of course the people of the Morbihan cliff-castles, and had no larger *oppidum* because their city was their fleet. Thereafter, it will have become more widely diffused in western Britain; and presently, somehow, it was introduced to south-west Wessex, that is, to Dorset and its borders, where Maiden Castle itself provides its grandest illustration. Wheeler set out his considered view of the matter further in ANTIQUITY for March 1944 (50-2), in reply to queryings by Mr Colin Gresham (*ibid.* June 1943, 67-70). His original idea, that its introduction at Maiden Castle was due not to any pre-Caesarian trade—Dorset has no tin nor anything like it—but to warlike refugees from the Venetic defeat by Caesar, in 56 B.C., establishing a strong minority rule over the previously stagnating Dorset natives, is there offered as a thing suggested not without reasons, and appearing to its author distinctly probable, but not insisted on if you do not like it. The same very disarming attitude is presented, with the same entire genuineness, in the new book. But by the new book itself, and of course inevitably, the case is altered somewhat.

We can now compare the pottery evidence from both sides of the Channel, presented by the same hands. How many readers of the Maiden Castle Report, fifteen years ago, did not in some degree fancy that it would amount to more than now it seems to? That of course was only their *ignotum pro magnifico*; but now nothing found has been left *ignotum*: the evidence is on the table, in no way over-pressed, and treated in every way with fairness. What does it in fact amount to? First, it upholds, in a general way, Caesar's testimony for the trade connexion, down to his time, between Brittany and Britain opposite, which must primarily mean Cornwall. Let more be dug up from Cornwall by all means; but so far so good. Secondly, it can show that 'isolated traits and tricks of the Breton craftsmanship' in pottery (p. 37) were transferred variously to parts of southern Britain, and that the Maiden Castle area of Wessex, and places outside it here and there, received thus the countersunk handle and the internal rim-groove. Actually, the handles could for that matter have been invented in Britain first, and the rim-groove imitates a corrugation used for rim-strengthening on vessels made of sheet-metal, which the potters of either region could have copied first, or both together; nevertheless, they do represent connexion with Brittany of some kind. At Maiden Castle these traits appear first with another, the bead-rim, which begins to be found only when the defences there were rebuilt in multiple form. In Cornwall or any district near or like it, all this would only represent the connexion we know already, that of the pre-Caesarian trade. But who would trade with agricultural Dorset? So it cannot represent that; people bringing sling warfare, multiple ramparts, and Breton pottery-traits to Dorset must have come there not because they wanted to, but because they were driven there. Dead-reckoning for the ensuing phases at Maiden Castle back from its eventual Roman conquest, about A.D. 45, brings one to the middle years of

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the 1st century B.C., therefore to the time of Caesar, and Caesar's conquest of Brittany in 56 B.C.; therefore to the driving out of Venetic refugees from Brittany; therefore it should be these, or some of them, who were driven to Dorset; therefore these ramparts and traits in Dorset must be explained not by the trade, as farther west, but by flight from the man who stopped it. This last, of course, is more than the evidence by itself amounts to; it is a personal suggestion offered for quite optional acceptance. What the evidence amounts to is that the Dorset region, somewhat individually yet broadly like other south-westerly British regions, had a connexion with Brittany of some kind, within the 1st century B.C. and not too far from its middle, since the dead-reckoning cannot (one agrees) be stretched more than twenty or thirty years farther back than Wheeler stretches it.

Thirdly, however, the evidence now declared from north France shows that apart from those traits, which clearly are distinctive of Brittany, and from traces of remoter Atlantic-coast connexions there of which in the 1st century there seems little left, the whole of the ceramic practice of northern France in the phase before Caesar was derived from the earlier La Tène work best known from the Marne. It normally used the potter's wheel, and its products fall geographically into only two broad groups—a Belgic and a western, which are distinct specifically but generically quite alike. The western group, essentially, is the pottery of Le Petit Celland, Artus, and (more or less) Vieux-Passage and Kercaradec. And now, fourthly, come the British correspondences. To the Belgic group corresponds that of our Belgic south-east. To the western, however, corresponds not that of Maiden Castle, with bead-rims typical, but the imported 'class B' ware of Hengistbury Head (p. 47: no. III of these Reports, 1915), with none at all. One bead-rim indeed was found at Le Petit Celland (fig. 9, 70), and some few are known from Brittany; from the Castel Coz cliff-castle (pp. 108-9) and from some graves (fig. 24). And if this is not many, yet, after all, bead-rims in general are only imitations of the rims of metal vessels. Venetic refugees could have brought such vessels to Maiden Castle, and had them imitated there, while their normal pottery was left behind. Or could they? At Hengistbury, at the joint mouth of the Avon and the Dorset Stour, that pottery does appear. Either (p. 47) it was brought by yet other Caesarian refugees, or else by pre-Caesarian trade. If by refugees, it should have been brought to Maiden Castle also: its absence from that great stronghold, despite the metal vessel doctrine, strains the refugee theory very badly. But if (as I believe) it was brought to Hengistbury by the pre-Caesarian trade, that trade did come to non-metal-bearing Wessex, and agricultural Dorset cannot have been left outside its range. Moreover, the Breton internal rim-groove and countersunk handle appear both at Hengistbury and in Dorset. The rim-groove appears also in the adjacent Glastonbury and south-western British culture (pp. 58-60); and there are elements in that culture's decoration of pottery, too, which seem to have come from Brittany (*Maiden Castle*, 216-8). We are thus taken back to Cornwall, where the pre-Caesarian trade-connexion with Brittany is certain anyway. Why cannot that connexion account for all these things? Why need Dorset and the Maiden Castle multiple ramparts be kept out of the picture, unlike all our other multiple ramparts, and accounted for separately by refugees? Traders coming to Hengistbury can have come to Dorset ports too—Poole and Weymouth: the 'agricultural' argument against is really only one from silence, that is, from too little port-side excavation. Maiden Castle is not a port; its pottery therefore is influenced by the trade without including actual 'class B' imports. And the dead-reckoning for the date of its ramparts could well allow them another twenty years—c. 75 B.C., in the trading period, instead of 56.

I suggest, then, that with the publication of the new Report we can give the refugee theory at Maiden Castle up. Historically, though truly attractive, it quite lacked warrant: to read Caesar (*B.G.*, iii, 16), no Veneti escaped his victory at all. Archaeologically, as

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Mr Gresham suggested in the article above recalled, it was never really necessary. Wheeler himself has very frankly left us free to take or leave it. And lastly, it has served its purpose. It began to do that right back in 1935, when it was only an embryo in Wheeler's mind. For it was this theory—of course it was—that first decided him to follow up his Maiden Castle work by doing one of the best things for European archaeology that he has ever done: conceiving, preparing, mounting, and carrying out, to the brink of war, the expedition recorded in this volume. Miss Richardson supported him sturdily, and the book is fittingly issued under both their names. It is superfluous to praise its finish, its style, its fine drawings, Mr Cookson's photographs. The work it records makes a noble climax to one of the biggest records of twenty-one years in any archaeologist's life—Mortimer Wheeler's record between the first war and the second. And now to end as I began. It is France, really, that this expedition to her shores has benefited most. If France would make her right response to the lead that it has given her, Wheeler would have achieved a record bigger still. Won't she—for her own sake—say yes?

Book Chronicle

- AN ARCHAEOLOGY OF SOUTH-EAST ENGLAND. By GORDON J. COPLEY. pp. 324 (41 line diagrams), 27 plates. *Phoenix House, London, 1958. 50s.*
- EXPLORING VILLAGES. By JOSCELYNE FINBERG. pp. xvi + 253 (60 text figures), 8 plates. *Routledge and Kegan Paul, London, 1958. 28s.*
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- THE KENSINGTON STONE: A MYSTERY SOLVED. By ERIK WAHLGREN. pp. x, 228, end-paper plates. *The University of Wisconsin Press, Madison, Wisconsin, 1958. \$5.*

[continued on p. 186]

Some Early Christian Remains in Southern Moravia

by JOSEF POULÍK

Dr Josef Poulík, Director of the Archaeological Institute at Brno of the Czechoslovak Academy of Sciences, describes here the archaeological discoveries made at Staré Město and Mikulčice in Moravia which reveal flourishing Christian communities before and in the time of the mission of St Cyril and St Methodius in 863 A.D.

CZECHOSLOVAK historians have for some decades devoted attention to the problem of the rise and development of the early feudal state of Greater Moravia which flourished under the princes Mojmir, Rostislav, and Svatopluk in the 9th century A.D. Several localities in Moravia and western Slovakia appear to have been focal points of this state but the majority of historians favoured the view that the principal fortress of the Mojmiride princes stood on the right bank of the river Morava at Staré Město near Uherské Hradiště. There are 12th century mentions of a *villa* Veligrad in this area and this name clearly preserves the tradition of an important fortress.¹

Those engaged in research on the Greater Moravian state have in the past relied principally on Frankish and Bavarian documentary sources, and to a lesser extent on legends of the Mission of SS. Cyril and Methodius, who were sent to Moravia from Byzantium at the request of Prince Rostislav. Moravians are first mentioned in documents in the year 822, at the well-known Council of Frankfurt, but this and other written evidence is insufficient to provide a full picture of the life and culture of central Europe in the 9th century. Archaeological methods have in consequence assumed special importance in the solution of these complicated problems.

Moravia is rich in memorials of its early feudal past. Fortifications, settlements, and burial grounds are all known but an unfortunate lack of financial support had until recent times prevented their thorough examination. When in 1948 more favourable economic circumstances permitted extensive excavations to be undertaken attention was first concentrated on the Staré Město area. Graves containing rich gold and silver ornaments had come to light here from time to time from the end of the last century onwards. Between the wars a devoted amateur archaeologist, Antonín Zelnitius, preserved many of these objects in the local museum, where they were examined by Prof. Lubor Niederle, the distinguished Czechoslovak Slavonic scholar. He concluded that they were Byzantine and Oriental work of the late 9th and early 10th century.

The 1948 excavations at Staré Město were carried out by the Brno branch of the Archaeological Institute of the Czechoslovak Academy of Sciences under the direction of Dr Hrubý, head of the Prehistoric Department of the Moravian Museum at Brno. The first site to be examined was that known as Na valách.² The immediate discovery of numerous graves, aligned from west to east and containing gold and silver ornaments, suggested that the remains of a church might exist in the vicinity. This was verified in the spring of 1949 when the foundations of a small church (37 ft. long) with rectangular nave and horse-shoe shaped apse were excavated. The building was of masonry and was

¹ Staré Město, Uherské Hradiště, and Veligrad mean Old Town, Magyar Castle, and Great Fort respectively. The form Veligrad is archaic. (Translator's note.)

² Na valách can be translated as The Dykes or Ramparts.

surrounded by so many graves that in places they formed a seven-fold layer. The graves contained a great variety of gold and silver jewellery, typically Slavonic pottery, and iron axes, spears, swords, and other weapons. Before the lively discussions initiated by these discoveries had died down news reached Brno that gravel-working at Staré Město had revealed, and was destroying, further graves and buildings about one mile west of Na valách. An emergency excavation in September 1949 resulted in the identification of the foundations of another church, somewhat larger than the first one (49 ft. in length) and possessing a narthex. It was likewise surrounded by graves with gold and silver ornaments, but though the narthex itself contained graves the total number of burials was much smaller than at the Na valách site. It appears that each of these churches had its specialized function.

The discovery of two stone churches at Staré Město is surprising in view of the opinion held hitherto by a number of historians and art-historians in Czechoslovakia and abroad, that the Slavs north of the Danube possessed only timber buildings, and that their culture was both simple and entirely dependent on foreign sources. Though there is not yet unanimity concerning the age of the newly discovered buildings archaeologists are generally agreed that they belong to the last third of the 9th century. During this period the Moravian state enjoyed its greatest prosperity, and the missionary activity of SS. Cyril and Methodius also falls within it.

The Staré Město discoveries re-opened discussion of the problem of the Greater Moravian capital. While some workers took them to support the view that the principal castle of Rostislav and Svatopluk was none other than Veligrad, and that Staré Město represented its site, others (particularly archaeologists) emphasized the need for further evidence. It had certainly been established that an important Slav settlement occupied the Staré Město site in the 9th century, that it possessed its own industry and jewellers' workshops, and that a flourishing trade existed with Byzantium and the West. What could not be excluded was the possibility of further discoveries of comparable importance, perhaps likewise on the River Morava or in the lowlands of south Moravia on the River Dyje. And indeed such a discovery did take place a few years later.

As early as the last century mentions occur in specialist literature of a fortified site Valy (i.e. Ramparts) on the lower Morava some twenty-five miles south of Staré Město, near Mikulčice. The distinguished Moravian archaeologist Dr I. L. Červinka considered that Valy belonged to the 11th and 12th centuries A.D. In 1954, following the successful work at Staré Město, exploratory trenches were dug on the north side of the Valy fortifications. Several graves and the traces of a stone building were revealed almost immediately. One of the graves yielded silver ornaments and two necklaces, which were assigned to the beginning of the 10th century. As the graves had been dug clear of the building it was considered probable that it represented the remains of a 9th century church. A systematic excavation of the site was accordingly undertaken in 1955 under the author's direction.

First to be exposed were the foundations of a small stone church with a rectangular presbytery and with a sacristy on the north side (FIG. 1A). An older building could be traced under this church (FIG. 1B), and a little further east lay fragments of another church (FIG. 1C) whose exact plan could not be ascertained. Within its compass, however, were included the graves of two nobles which contained heavy iron swords of a late Frankish pattern—J. Petersen's type K. Another grave yielded objects cast in bronze, of the kind called 'Avar' work. These circumstances indicate that the church C dates from the early 9th century and is thus the oldest known Christian building in central Europe.

Some 200 graves are associated with church A at Mikulčice. The ornaments recovered from them are generally silver, gilt, or bronze. They resemble those found at Staré Město.

SOME EARLY CHRISTIAN REMAINS IN SOUTHERN MORAVIA

A particularly interesting grave, that of a nobleman, contained two pairs of spurs. One pair were of iron and the other of bronze heavily gilt (PLATE XIX). The latter had side pieces richly decorated with human faces in relief. Accompanying the spurs was much other finely gilt metal work. At the west end of the church was discovered the grave of a six-year-old boy. Buried with him was a leather belt with a silver-gilt endpiece; the outer surface is finely ornamented and on the inner is engraved the figure of a priest in a ceremonial attitude (PLATE XVIII (*a & b*)). This is the first object of its kind to be discovered among the Slavs north of the Danube and despite indications of some Byzantine and Western influence we are compelled to assign a local origin to it. Both this belt ornament and the gilt spurs show that the church site was reserved for the graves of the ruling feudal class. The graves date mainly from the second half of the 9th century but it appears that the church was built before the arrival in Moravia of SS. Cyril and Methodius in 863.

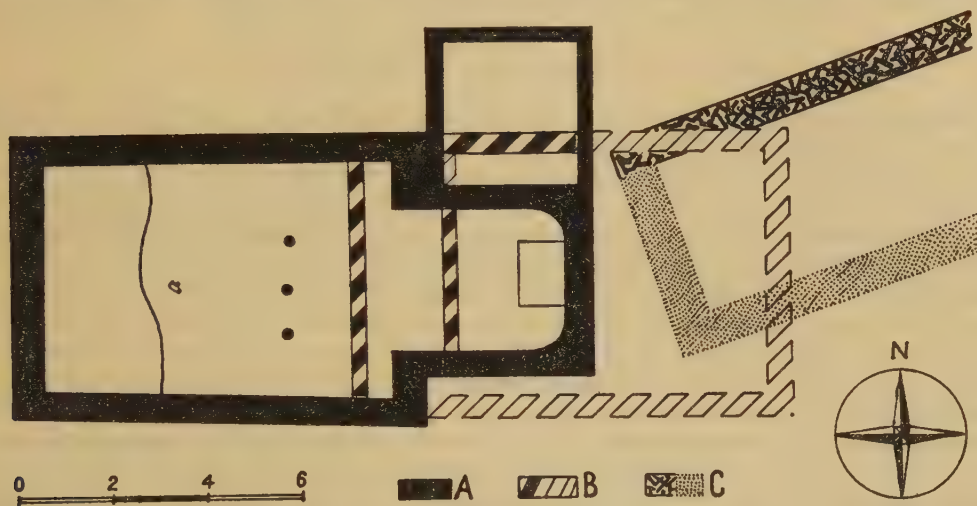


FIG. 1. Mikulčice. A. Church. B. building with mortar floor. C. earliest church.

The Mikulčice excavations were continued in 1956–57 towards the centre of the fortifications. They brought to light the foundations of a further church, with three aisles, some 120 ft. long and 33 ft. wide (FIG. 2). Over 400 burials were found in its neighbourhood, and the aisles and narthex contained remains of masonry tombs. Many of the skeletons had gold and silver jewellery by them while heavy two-edged swords were found in the graves of six noblemen. Of special interest was the discovery of a gold coin of the emperor Michael III dating from the years 856–66. This fact, together with other archaeological evidence, establishes that a great church building (at present the greatest known north of the Danube) stood here at the time of the mission of SS. Cyril and Methodius. The three aisles were, in all probability, already in existence in the first half of the 9th century.

The results of the Mikulčice excavations do not so far provide conclusive evidence concerning the function of the fort in the political organization of Greater Moravia. The present acropolis, occupying some 15 acres and containing both wood and masonry in the remains of its massive defence works, superseded a Slavonic timber stockade of the 7th–8th century. This has been dated by the discovery of spurs with hooks—the ‘Hackensporen’ of German archaeologists. The site was thus not that of a simple agricultural settlement but rather the seat of a military leader. The later fortifications belong

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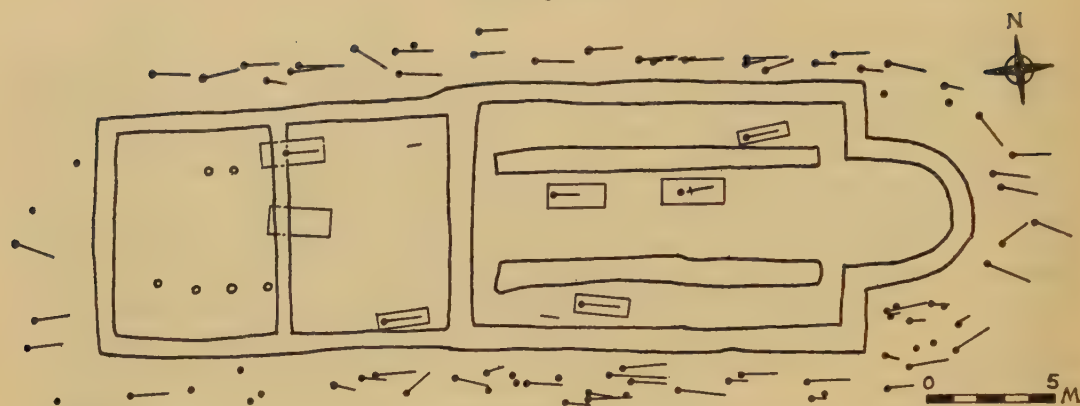


FIG. 2. Mikulčice. Church with three aisles.

to the end of the 8th or the beginning of the 9th century and are the work of Prince Mojmir or one of his predecessors. The acropolis was occupied by an aristocracy which buried its dead in and around its churches (indeed, we may yet hope to come upon a third church, and the prince's residence itself) while an extensive 'castle-town' or *subburgium* immediately adjacent to it contained a burial ground for the ordinary people. Traces of stone buildings have been discovered in this *subburgium*, and it is quite possible that artisans' workshops will come to light here as at Staré Město.

It will be many years before we shall possess a comprehensive view of the Valy complex, though eventually our knowledge of the rise and development of the town in central and western Europe will be substantially increased through it. At present we can say with certainty that the right bank of the Morava at Mikulčice was the site of an important administrative and economic centre of the Greater Moravian state of the 9th century. Early in the 10th century increasing Magyar pressure began to disrupt its organization, and Valy was one of several sites to be abandoned. Political power was gradually transferred westwards, to the Přemyslide state emerging in Bohemia.

Both at Staré Město and at Mikulčice our excavations have demonstrated that Greater Moravia possessed a developed administrative system based on military strongholds which at the same time served as centres of trade and religion. Artistically we see the Moravians of that time capable of a strong and distinctive adaptation of Byzantine and Western themes, and we may be sure of the preponderance of native influence in the growth of their political institutions also. In a wider context the discovery of church buildings and of artistic work on this scale implies the existence in the 9th century of a region of distinctively Central European culture of high level. Christianity, the progressive element of the period, was an important component of this culture, and it is thus especially gratifying to have proof of its presence in Moravia as early as the first half of the 9th century, before the advent of SS. Cyril and Methodius.

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Archaeomagnetism: A Preliminary Report on Britain

by R. M. COOK and J. C. BELSHÉ

In this article Mr R. M. Cook, Reader in Classical Archaeology in the University of Cambridge, and Mr J. C. Belshé, Senior Assistant in Research in the Department of Geodesy and Geophysics in the University, discuss the results of the researches into the archaeological significance of remanent magnetism dating which they have been doing during the last five years. Their work continues, and if any reader knows of suitable material they would be grateful if he would get in touch with them through Mr Belshé, Department of Geodesy and Geophysics, Madingley Rise, Madingley Road, Cambridge (Telephone: Cambridge 2103).

I. INTRODUCTION

THE existence of permanent magnetism in igneous rocks and fired clays has been known since the middle of the 19th century. Several attempts have been made to use it for geological and archaeological purposes (see Bibliography). The inquiry reported here was begun in 1953 in ignorance that Prof. E. Thellier was already working along similar lines in France.¹ Later we were able to confer with him, learning much from his technical methods, and in return offering him additional material for study. The collaboration has also afforded an opportunity to check the accuracy of our own measurements.

2. THEORY

Many rocks—that is, according to geological usage, stones and natural clays—contain oxides of iron and are feebly but appreciably magnetic.² The character of their magnetism varies according to their nature. If igneous or formed by processes of slow sedimentation in water, a rock may have a natural remanent magnetism which is uniform throughout the layer or deposit and also in the direction of the magnetic field in which it formed. If formed in more disturbed conditions, such as is the case with glacial clays, this natural magnetism may be quite random. Working and moulding material from a uniformly magnetized clay deposit will also tend to make its magnetism random.

When subjected to heat the magnetism of a rock decreases and vanishes at what is called the Curie-point, a temperature usually in the range 500–600° C. If the rock is then cooled below the Curie-point it becomes remagnetized, and the new magnetization has the direction and proportionately the intensity of the magnetic field of its environment. The activities of man have often resulted in clays or stones being fired in this manner.

The effects may be complicated if the material is fired more than once, since a partial magnetization can be acquired even at temperatures below the Curie-point.³ If the last

¹ See Bibliography, nos. 5, 6, 7.

² This ranges from 10^{-2} emu/cm³ downwards to intensities of magnetization below any useful level of measurement (approximately 10^{-8} emu/cm³). In most of the specimens we have studied the magnetization lies in the range 10^{-2} – 10^{-6} emu/cm³. This is comparable to the magnetization of igneous rocks.

³ This may reflect the presence of several magnetic components in the material since each will have its characteristic magnetization. It also reflects the balance existing between the magnetic forces favouring alignment of the elementary particles of matter and the kinetic forces disrupting this alignment. Since these kinetic forces are directly proportional to temperature, a small increase in temperature may cause a small loss of magnetic coherence. This effect is, of course, most marked in the temperature range lying near the Curie-point.

heating reaches the Curie-point, traces of any preceding magnetization vanish. But if the last heating does not reach the Curie-point nor yet the temperature of a previous heating, the rock may possess two (or more) remanent magnetizations and the apparent magnetization will be an intermediate one. Such magnetizations can be detected experimentally by heating a specimen of the rock when it is isolated from any magnetic field. If, for instance, the rock was first heated to 800° C. and later heated again to 400° C., any second magnetization can be destroyed by heating slightly above 400° C. and allowing it to cool in zero field, that is in isolation from the earth's field or other magnetic disturbances. The first magnetization can then be ascertained, and by comparison with the record of the intermediate magnetization the second magnetization can be calculated.

The magnetic field which causes the remanent magnetism of fired rocks is normally that of the earth; this was only to be expected and seems confirmed by results. Other causes are comparatively unimportant and might be deduced if they have occurred in some instances. (1) A strike of lightning may completely remagnetize a specimen. If the direction the electric current might travel is random, then the chances that its resulting magnetization would fall within the possible range of direction of the geomagnetic field are about 1 in 200 and the chances that two distinct structures of the same date should receive a similar magnetization are negligible. (2) Local, natural, magnetic anomalies might distort the earth's field; but those of such magnitude as to be significant for our study should be known from other geophysical investigations and are unlikely to have changed with time. (3) The proximity of iron objects—man-made anomalies—might have a limited effect; but any such objects would hardly be large in size and since their effect decreases with the cube of the distance, samples from different parts of a structure should betray it. Besides, iron was relatively scarce before the modern era. (4) The daily variation in the earth's field, which naturally affected the magnetization of specimens, is usually under $10'$, but occasionally as much as 1° . This variation cannot be discounted, though it is usually smaller during the night, when (as we suppose) ancient kilns were usually cooling through their Curie-point. But it sets a useful limit to the accuracy to be attempted.

At any one place the earth's magnetic field varies with time in direction and intensity, so that the remanent magnetization of a specimen should be characteristic of some particular time or times in the earth's history at that place. As an example of this variation in direction of the magnetic vector, FIG. 1 shows the observed change in the annual mean direction of the earth's magnetic field at London from a little before 1600 A.D. to the present day.⁴ Since such observations do not go back beyond the 16th century A.D., it is clear that much new information about the earlier history of the earth's magnetic field can be obtained by measuring the direction of the remanent magnetization of suitable rocks, provided that we know the orientation of these rocks when they received their magnetization.

3. AIMS AND APPLICATION

Our aims have been twofold. First, we proposed to take samples of fired clay or stone which acquired their remanent magnetization at dates which were known from historical records or archaeological contexts: in this way we hoped to construct for geophysicists a curve of recent variations of the direction of the earth's magnetic field. Secondly, if such a curve could be constructed, we intended to date by their remanent magnetization samples

⁴ The direction of the magnetic vector is described here by two angles, those of inclination and declination. The angle of declination, familiar to those who use an ordinary compass, is the horizontal angle between geographic north and the vertical plane that passes through the magnetic vector. The angle of inclination (or dip) is the vertical angle between the vector and the horizontal plane (or tangent) of the earth's surface at the point of measurement.

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which archaeologists and historians could not date by contexts or written records. Archaeological remains that possess a datable remanent magnetism include pots of fired clay, kilns, hearths, and other structures of clay or stone that have been fired whether by accident or design.⁵

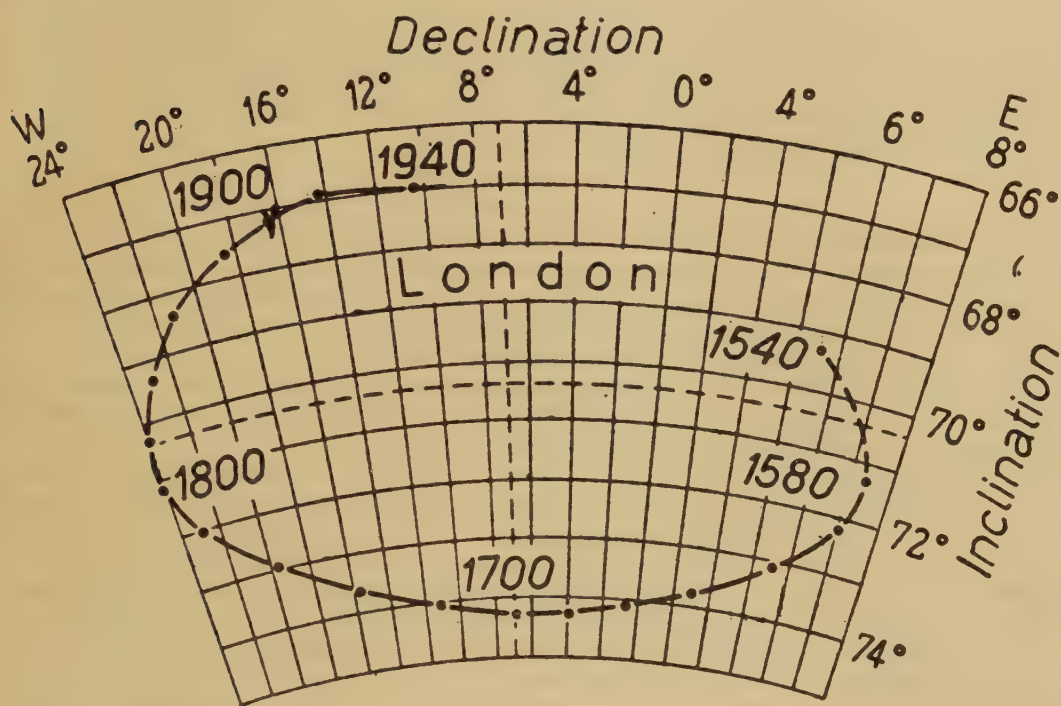


FIG. 1. Observed changes in the annual mean direction of the earth's magnetic field at London since 1580 A.D. The continuation to 1540 A.D. is extrapolated from observations at Rome. (After L. A. Bauer.)

Attempts to use archaeological remains have, of course, been made several times in the last sixty years. But before Thellier's work none was successful. The reason is that the earlier investigators restricted themselves to pots. They conceded that they could not immediately determine from a pot what was its angle of declination (or compass-bearing) at the time when it was fired. But they assumed that the angle of inclination (or dip) was fixed, since pots stood level on their bases when stacked in the kiln. This may be true of glazed ware, where only the unglazed base or spurs had contact with other objects; even so, a pot may balance on its base and yet its axis be several degrees out of the vertical.⁶ But most ancient pots were not glazed and, since there was no danger of their fusing together, they could be stacked at any angle in the kiln. Tests that we made on a few random

⁵ There are also some dated flows of lava (see Bibliography, no. 2).

⁶ In the last two years the Research Laboratory for Archaeology and the History of Art at Oxford has been examining the direction of the remanent magnetism in Chinese porcelain. The results, now published by M. J. Aitken in *Archaeometry*, 1 (1958), 16-20, show a fair consistency.

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specimens of ancient Greek pottery suggest that they were so stacked.⁷

For this reason we decided at the beginning of our inquiry to concentrate on structures which had not been disturbed since they were fired and magnetized. The walls and bottoms of kilns have proved very satisfactory. In firing they have been subjected to a heat of not less than 800° C. Since they are commonly built in pits about 2 ft. deep, their lower parts are often well preserved, and because of their shape and nature it is fairly easy to decide whether those parts have shifted since firing.⁸ Their period of use was generally short, so that the remanent magnetization, if not that of the last firing, accumulated over only a few years. Lastly, kilns are fairly numerous in this country, especially of the Roman period, and normally several examples are excavated every year.

4. METHODS

In the measuring of samples there are two technical problems. First, when a sample has been chosen and before it is removed from the site, its horizontal and vertical bearings must be marked, so that its original orientation can be ascertained when it is measured in the laboratory. Secondly, a suitable instrument is required for making measurements of the remanent magnetization. Both the marking of the bearings and the measuring need to be accurate; in each case the limit we set ourselves was one degree. Suitable techniques had already been developed by students of the natural magnetization of rocks, but during the course of our work we learnt to modify them.

We have used three methods of marking the orientation on samples before removing them. Each method has its own special advantages, but the third is generally the most satisfactory and we now use it whenever we can.

1. If a sample already presents a plane surface, a horizontal line can be scratched or painted on it with the aid of a spirit level. Next, the angle of dip of this surface is determined by an inclinometer along a line at right-angles to the horizontal line. Lastly, the bearing of the horizontal line is taken either by a compass or a theodolite. This method is most useful for some brittle specimens such as tiles.

2. If a sample does not have a suitable flat surface, a small tripod can provide a substitute. This tripod is a plate mounted on three legs, two of which screw with a fine thread. By adjusting the screws the surface of the plate can be made level. A bearing is taken along two fixed points on the plate. The places where the legs touch the sample must be marked and the adjustments of the screws recorded. Then in the laboratory the original orientation can be recovered by adjusting and replacing the tripod in the same position as before.

3. The method we prefer is to cast the sample in a duraluminium mould $4\frac{1}{2}$ in. or 8 in. square and from $2\frac{1}{2}$ in. to 4 in. high. The chosen sample is prepared by cutting round and trimming its sides. The mould is then fitted round the sample and levelled with a spirit level. Next plaster is poured in, enclosing the sides and top of the sample. The top surface of the plaster is carefully made level with the top of the mould, so that it too is horizontal.

⁷ The determination of the magnetic vector suggests two limited applications in the study of sherds of wheel-made pottery. Since the wheelmarks are regularly in a horizontal plane—that is, parallel to the plane of the base of the pot—the vertical angle of a sherd may be ascertained. It should then be possible to discover, by comparing the inclination relative to the horizontal plane of the wheelmarks, whether two sherds can or cannot be from the same pot; if they are, the inclination should be the same. Further, if two sherds are from the same pot, their respective declinations should show their relative longitude on the curved surface of the pot. But both these applications are likely to prove difficult in practice.

⁸ The cracking and crazing visible in the surface of the walls of many well-preserved ancient kilns probably occurred in the firing of the kiln and so may be, in part at least, earlier than their present magnetization. We judge from observation of the kiln of Roman-British type which F. J. Watson made and fired at Wattisfield in 1956.

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When the plaster has set, an alidade is laid on its surface and a bearing is taken along the line of the alidade and scored on the plaster. After the sample has been removed by cutting away below, the bottom too may be filled in with plaster, so that the sample is completely enclosed in a plaster cube. Specimens prepared by this method can be transported more safely and are more convenient for the magnetometer we now use.

In taking a bearing along the line of the alidade, a theodolite is more reliable than a compass. The theodolite can be used to determine true north by observing the sun angle at the time read by an accurate chronometer or by resection from objects identified on a map. If a compass has to be used, it should be set some distance away from the sample, so that the sample's magnetic moment does not disturb the compass setting. Usually the bearings of several samples can be taken from one station. It is desirable to have at least five samples, symmetrically disposed, from any one structure.

The measurement of a relatively weak magnetization can be done with an astatic magnetometer of the type described by Blackett (Bibliography, no. 11).⁹ It consists essentially of two magnets of equal intensity mounted in anti-parallel, and set one a few centimetres vertically above the other. Such a system nullifies the directing effect of the earth's magnetic field acting on a single magnet, since the positive effect of that field on one magnet is balanced by its negative effect on the other. This magnet system is hung on a weak torsion fibre which, in the absence of any disturbing forces, determines the rest position of the system. The method of measuring the sample is to bring it, set in some particular orientation, below the lower magnet of the pair. Since the force of the sample's magnetic field decreases by the cube of the distance, the magnetic effect of the sample is much greater on the lower than on the upper magnet. This force causes a deflection of the magnet system against the restoring force of the torsion fibre. From the deflections obtained for several orientations of the sample the magnetic vector best representing the whole sample can be calculated and its direction relative to the orientation plane determined.

At first, to suit the magnetometer we were then using, measurement was done only on small cylinders or discs $1\frac{3}{8}$ in. in diameter, cut from the samples by non-magnetic stone-cutting tools.¹⁰ This process had four dangers—inaccuracy in transferring the marks of the orientation to the cylinder, wobbling in the cutting, the shortness of the orientation marks, and possibly partial remagnetization due to heat applied in drying or generated in hardening. Review of the data obtained by late 1955 confirmed our fears that this method was too imprecise; there was fair accuracy in the measurement of inclination but less for declination. The obvious remedy was to measure the whole sample.

⁹ Another type of magnetometer is used by Thellier and the Research Laboratory at Oxford. Here the sample is placed within a set of coils connected to a galvanometer; and, as the sample is rotated, the electric currents which its magnetization generates in the coils are recorded by the galvanometer.

¹⁰ The cutting was done by an electric drill with a diamond corer, and water was used to clear away cuttings and for cooling. This process was satisfactory for hard rocks, but specimens of burnt clay from the walls of kilns are much softer and usually disintegrated in cutting, unless they had been specially prepared. Since it may be useful for other purposes, we describe the method of preparation that we finally adopted. The specimen was allowed to dry, heated to about 100° C. to expel more of its water, and set in an expendable container. Next it was put in a bell-jar, the jar sealed, and the air pumped out by a vacuum pump for about three hours. Then a solution of Marco-resin was introduced into the container till the specimen was covered. After it had soaked for about fifteen minutes the air was let into the bell to assist in forcing the solution to penetrate the specimen. The container was then stood in cold water to reduce the heat given off in the setting of the resin. When it set, the specimen became hard and tough and could be drilled quite safely.

The formula we used was 1 lb. Marco resin SB 280, 44 c.c. Monomer C, 18 grams Catalyst paste H, 14 c.c. Accelerator E.

For our purpose it was convenient to enclose the specimen in Marco resin. If the surface of the object is to be kept free of resin, then the Accelerator should be omitted from the solution and after the interior has been permeated the surface should be wiped clean with Monomer C.

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A visit about this time by Prof. E. Thellier of the University of Paris was very valuable. In spring 1956, on the discovery of the best preserved kiln that had yet been available to us,¹¹ Prof. Thellier returned to assist in the taking of samples which were to be measured in both laboratories to compare our accuracy. Observation of Thellier's methods convinced us of the merits of casting samples *in situ* and this is now our normal method.

To measure the new cubical blocks a special magnetometer was constructed. The support for the sample is a horizontal table that can be rotated about an axis in line with the axis of the magnet system. In the middle of the support is a square recess which just accommodates the sample. This provides the means for aligning the cube edges with the magnetometer's axes. Large current-carrying coils surround the magnetometer and sample-holder to cancel the earth's magnetic field and avoid induction effects this might cause in the sample. The data obtained, from measurements of all six sides, are plotted on a large stereograph where the vectors can be read to $\frac{1}{2}^\circ$. The data could also be computed mathematically.¹²

PLATE XX A-D shows the remains of a kiln being sampled, a block which has been removed from the mould,¹³ and the magnetometer with a sample under it.

5. RESULTS

The Table summarizes the results of our measurements of samples collected or received up to August 1957. To make comparisons easier, values for inclination have been adjusted to the latitude of Cambridge in accordance with the data given on Admiralty Chart 5383; we have assumed provisionally that the variations due to latitude only have remained constant (pp. 176-7).

The precision of these results varies. One reason is that the materials of which the structures were made are not all equally satisfactory. Another is that we have not had enough samples from some sites.¹⁴ But the principal reason is that we had to learn by experience; in our earlier work, as is shown by the omission of values for declination, we did not always orientate samples accurately in relation to true north. But later (for example at AGA, AGB, AGC, AGD and AL) the precision has reached the order of 1° (see Bibliography, no. 12). The bar between AF and AGA marks the change from our earlier to our present methods of collection.

Two structures, O and AGE, show notably anomalous results. O had been shifted by the roots of trees, but not enough to account for the lowness of the inclination. More likely explanations are: first, that the samples were bricks from the substructure of the kiln—the only part preserved—and when the kiln was fired the substructure may not have been heated to its Curie-point; secondly, there was above this substructure a grid of iron bars, which may have had a disturbing effect on any remagnetization of the bricks. AGE was

¹¹ Grimstone End (AGA in the Table).

¹² The result for the sample is obtained by relating this vector to the orientation the sample had *in situ*, and then determining the vector's direction with respect to true north. The result we give for any structure is the mean of all these vector directions. An estimate is made of the probable error from the scatter observed between the results of different samples.

¹³ At the top on the right is the reference number. Immediately below is the mark of the bearing along the line of the alidade; the arrow-head shows the direction in which the bearing was read. Further down the two parallel lines show magnetic north as determined by a box compass laid on the sample; the direction of north is marked by an arrow-head. See PLATE XX (c).

¹⁴ For example the kilns N and Qa were to be preserved, so that we could not remove the specimens we preferred; of the kilns Qb and R very little was uncovered; and of B only a small part had survived the bulldozer and exposure.

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a puzzling complex, and the samples from it (which were measured by Thellier, too) gave readings consistent in themselves, but in our opinion very improbable as records of the magnetic field of the earth. Perhaps the magnetization of this kiln was altered by a lightning strike.

Of the other structures AM must be left aside, since it is much the earliest and isolated in time. The remainder, which give generally consonant results, belong to two historical periods, the Roman and the later Medieval. For the Roman period we have enough good samples to allow reasonable certainty about the direction of the magnetic field in England. For the later Medieval period we offer only four structures. FIG. 3 sets out the average readings for declination and inclination of samples from ten Roman and the four Medieval structures. FIG. 2, with measurements of inclination only from thirteen Roman structures, shows the range of variation (or scatter) between different samples from each structure.



FIG. 2. Scatter in readings of inclination in thirteen samples from Roman kilns. Vertical lines show the scatter of the readings, horizontal the probable latitude in the archaeological dating.

Not all the Roman results are plotted in these diagrams; the selection has been made according to the condition of the structure and the faith we have in our collecting and measuring of the samples. But the inclusion of the omitted results, as the Table shows, does not alter the general trend. During the 1st to the 4th centuries A.D. the direction of the magnetic field shows a steady eastward motion and a shallowing of its dip. In the later Medieval period there appears to be a more rapid motion of increasing inclination as well as an eastward shift in the declination. Both curves conform to the pattern of clockwise rotation recorded by magnetic observatories, and the rate of change agrees reasonably with the observed change of about 2° in twelve years. This rate of change makes it advisable to be cautious in conjectures about the magnetic direction during the long earlier Medieval period, tempting though it is to fill the gap on FIG. 3 with a simple curve.

Our results, both from the Roman and the later Medieval periods, accord with those obtained by Theillier for inclination in northern France and the Rhineland (see Bibliography, nos. 5-6).

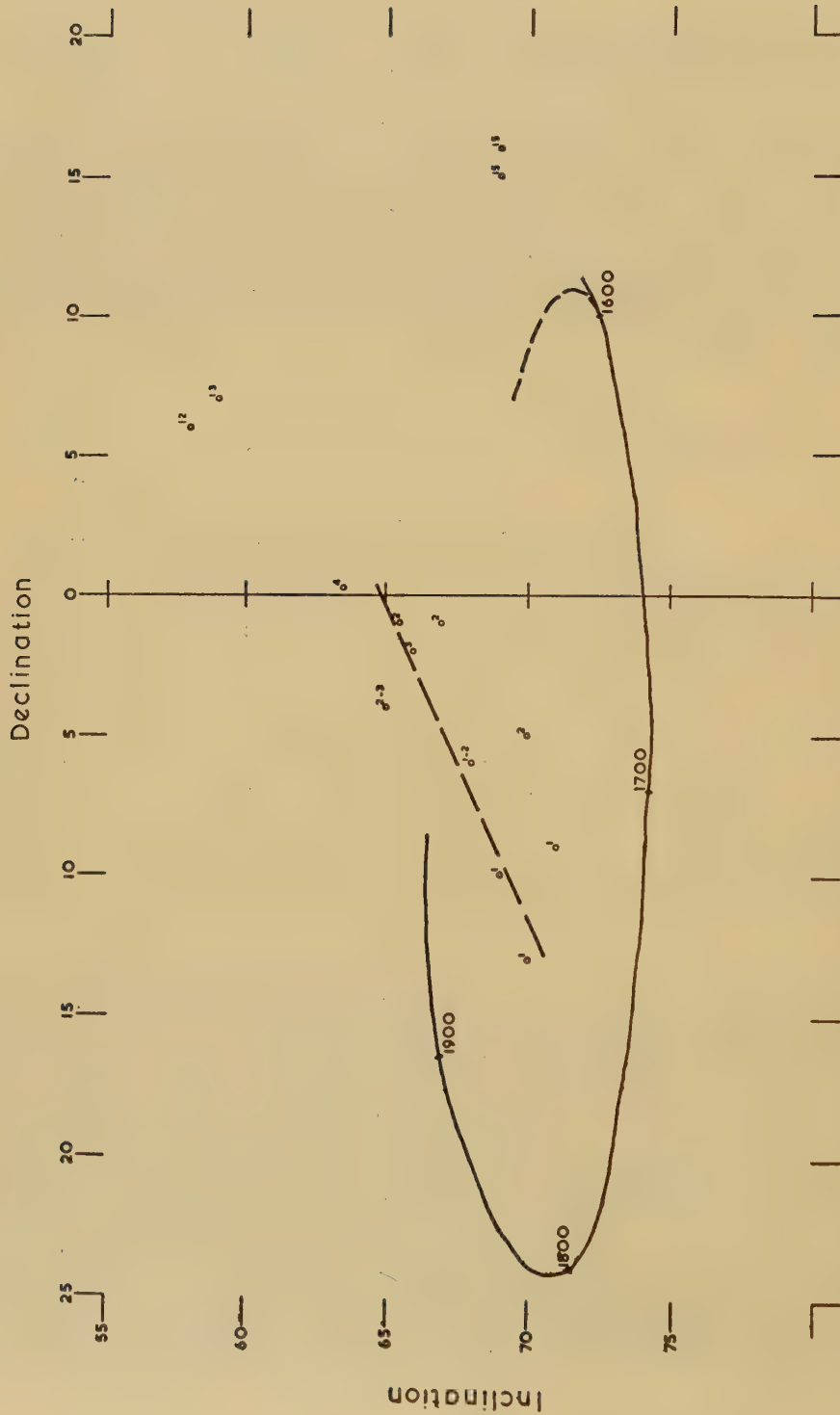


FIG. 3. Changes in the annual mean direction of the earth's magnetic field in England since the 1st century A.D. The continuous black line represents observations at London since 1578 A.D. and the extension in short dashes Bauer's extrapolations from observations at Rome (cf. Fig. 1). The rings show the average readings, corrected for Cambridge, of fourteen of our samples (see Table p. 176-7), and the line of longer dashes the trend for the Roman period.

6. DISCUSSION

On FIGS. 1 and 3 the record of direct observations, represented by a solid line, goes back only to the late 16th century A.D. Our points for the 15th century appear as a simple continuation of that line (FIG. 3). This conflicts with the direction suggested by Bauer (FIG. 1).¹⁵ But the relevant part of Bauer's curve (drawn with an intermittent line) is based on extrapolation of observations made in Rome in and after 1540, and he ignored or was ignorant of the observations of Borough and Norman which were made in London in 1576.¹⁶ Since the strong easterly declination about that time may reflect a centre of magnetic disturbance then situated in northern Europe, Bauer's extrapolation is dubious. It is only fair to remark that he himself, unlike some of his followers, did not consider it secure.

It has been known for many years that the earth's magnetic field is characterized by regional centres of magnetic disturbance. Their effects are more clearly exhibited by removing from the observed field that part which would come from the uniformly magnetized sphere most closely fitting the observed field. In 1945 there were seven major centres of disturbance and their regional extent was of the order of a thousand miles.¹⁷ From older observations it is known that such disturbances change their shape and magnitude with time and have slowly drifted westward. The processes causing them are thought to occur in the earth's core and it is their effect on the measurements made by a magnetic observatory which is responsible for the secular change of magnetic components at that observatory.

This regional character of the earth's field means that the results of a study such as ours cannot at once be extrapolated around the world. Even so, the results are more than local in their application and we can hope that, if enough work is done, it should become possible to make valid comparisons of data within regions the size of Europe. For the British Isles our confidence in extrapolating results such as are reported here is still greater.

As has been said, the rate of observed change in the direction of the magnetic components seems to be for Britain about 2° in twelve years. At this rate a measuring accuracy of $\pm 1^\circ$ or even $\pm 2^\circ$ should permit fairly precise chronological computation. If an accurate curve can be constructed, it does not seem over optimistic to expect that suitable material properly sampled and measured should be datable to a quarter of a century.¹⁸

The accuracy with which the secular change of the earth's magnetic field can be documented is limited principally by the uncertainty of archaeologists in dating their fired structures and, of course, by the supply of such structures. For the Roman period structures that are more precisely dated would be helpful. For the later Medieval period our evidence is gravely insufficient. But even more urgent is the need for material to fill the gap between the 4th and the 13th centuries A.D.

¹⁵ The diagram has often been repeated in text books.

¹⁶ These are marked by the beginning of the solid line on FIG. 3.

¹⁷ The number and extent of such disturbances depend of course on the criteria one chooses to define them. We are generalizing about the foci showing changes in the total magnetic intensity of several tens of gammas per year which have been in evidence for the first half of this century. See figs. 140-143 of Bibliography no. 13.

¹⁸ These estimates apply, of course, to our use of inclination and declination to define the total vector. It would be possible to limit study to one of these and still gain useful knowledge. For example FIG. 2 shows that the variation of inclination during the Roman period might suffice as an aid in dating. Another variable could be added, the intensity of magnetization, which Thellier suggests has recorded a 50 per cent decline during the last 2,000 years (see Bibliography no. 6). Study of intensity alone might be useful for establishing a coarse chronology (measurement to a 10 per cent accuracy would permit dating to about 200 years by Thellier's estimate). Used in conjunction with a study of remanent directions of samples taken *in situ*, the intensity could refine the measurements and decide ambiguities where the curves of direction intersect.

TABLE

Ref.	Location	Type of Structure	Excavators' Date	Condition of Structure	No. of Samples	Declination	Inclination	Comments
A.	Grimstone End, Pakenham, Suffolk.	Kiln ?	Anglo-Saxon	Probably scattered debris	4	Erratic		Probably not <i>in situ</i> .
B.	Grimstone End, Pakenham, Suffolk, (No. 19.)	Kiln	3rd-4th cent. A.D.	Fair	6	2° W (± 2)	66° (± 2)	See Fig. 3. See Fig. 2.
C.	Norton, nr Malton, Yorks. (R. H. Hayes and E. Whitley, <i>Roman Malton and District</i> Report no. 7, 41-2).	Kiln	Late 3rd-early 4th cent. A.D.	Good	1	10° W (± 4)	66° (± 4)	Inclination reasonable. See Fig. 2.
D.	York (Museum Gardens)	Floor	c. A.D. 120	Fair	2		60° (± 4)	Inclination shallow.
E.	Doncaster (Cantley, kiln 1)	Kiln	c. A.D. 110-170	Poor	2	Broke up		
F.	Doncaster (Cantley, kiln 5)	Kiln	c. A.D. 110-170	Poor	1	Broke up		
G.	Doncaster (Cantley, kiln 4)	Kiln	c. A.D. 110-170	Poor	1	Too weakly magnetized		
H.	Doncaster (Cantley, kiln 3)	Kiln	c. A.D. 110-170	Perhaps wall had shifted	1	5° W (± 4)	66° (± 2)	
J.	Doncaster (Cantley, kiln 2)	Kiln	c. A.D. 110-170	Fair (kiln had been remodelled once)	2	3° W (± 4)	59° (± 3)	Agrees with date in 2nd cent. A.D. See Fig. 2.
K.	Rickingham, Suffolk (Garden House field).	Kiln	3rd cent. A.D.	Fair	2	5° W (± 4)	77° (± 1)	Inclination very shallow.
L.	Overwey, Tilford, Surrey: kiln 1. (A. J. Clark, <i>Surrey Arch. Coll.</i> , LI, 29-56.)	Kiln	c. A.D. 363-8	Poor	3	Erratic	78° (± 2)	Inclination very steep.
M.	Wattisfield, Suffolk (Mill House Farm).	Kiln	15th cent. A.D.	Fair	2	16° E (± 3)	69° (± 1)	See Fig. 3.
N.	Wattisfield, Suffolk (Foxledge Common: this kiln was later removed to the Wattisfield Potteries).	Kiln	2nd-3rd cent. A.D.	Good	2		76° (± 4)	See n. 14.
O.	Lowestoft (112 Yarmouth Road: probably Hewling Luson's first experimental kiln).	Kiln	c. A.D. 1756	Doubtful	3		49°	See p. 172.
P.	Chew Park, Somerset: (a) Roman lime-kiln . . .	Lime-kiln	Probably later	Fair	3		64° (± 2)	See Fig. 2.
Q.	(b) Medieval lime-kiln no. 2 .	Lime-kiln	3rd cent. A.D.	Fair	1	6° E	58°	See Fig. 3.
R.	(c) Medieval lime-kiln no. 3 .	Lime-kiln	12th cent. A.D.	Fair	1	Too weakly magnetized		
S.	(d) Oven no. 1 .	Flue	12th cent. A.D.	Fair	1	Too weakly magnetized		
T.	Sneyd Green, Hanley: (a) .	Kiln	c. A.D. 1300	Fair	2	7° E	59° (± 2)	See Fig. 3.
U.	Botesdale, Suffolk (prefab. site) .	Kiln	Late 2nd-3rd cent. A.D.	Doubtful	1	Erratic		
V.	Rickingham, Suffolk (Kiln Farm —see AI).	Kiln	3rd cent. A.D.	Only edge available	2		57° (± 4)	Inclination very shallow.
W.	Totternhoe, nr Dunstable .	Hypocaust		Fair	2	4° W (± 3)	65° (± 2)	See Fig. 3. See Fig. 2.
X.	High Wycombe, Bucks: (a) .	Flue	A.D. 150-200	Fair	3	Erratic		
Y.	Wattisfield, Suffolk (Hall Farm)	Flue	A.D. 150-200	Fair	1			
Z.		Hearth	Perhaps 3rd cent. A.D.	Poor	1	Too weakly magnetized		
AA.		Kiln			3	Too weakly magnetized	69° (± 1)	Inclination suggests 1st-2nd cent. A.D.

X.	(b) Benetfinks, London	Hearth	15th-early 16th cent. A.D. Later 1st-2nd cent. A.D.	Poor	1	15° E	69° (±2°)	See Fig. 3.
Y.	High Cross, Leicestershire: (a) Trench 50 (b) Trench 52 (c) Trench 31	Burnt area Burnt area Flue		Doubtful Doubtful Poor	1 1 2	6° W	68° (±2°)	See Fig. 3.
Z.	Woodhouse Hill, Studland, Dor- set: (a) (b) (c) (d)	Hearth Hearth Hearth Hearth	Probably 4th cent. A.D. Probably 4th cent. A.D. Probably c. A.D. 110 Probably c. A.D. 50		1 1 1 1	Too weakly magnetized Too weakly magnetized Broke up Too weakly magnetized Broke up Erratic		
AA.	Wattisfield, Suffolk (Grundle Farm: higher level—see AGE).	Kiln	Perhaps 13th- 17th cent. A.D.	Poor and perhaps disturbed	3	9° E	57°	Suggests 13th cent. A.D.
AB.	Downton, nr Salisbury (villa)	Hypocaust flue	A.D. 300-320		1		85°	Inclination too steep.
AC.	Thurgarton, Notts (Castle Hill). (<i>Journal of Medieval Arch.</i> , I).	Crucible?	Later medieval	Good	1	Too weakly magnetized		
AD.	Caer-Mynydd, nr Rhinwlas, Caer- narvonshire.	Hearth			1	Broke up		
AF.	Grimstone End, Pakenham, Sflk.	Kiln	c. A.D. 160-200	Good	1	2° W	64°	See Fig. 3.
AGA.	Canterbury II	Tile-kiln	c. A.D. 350	Good	13	1° W (±2°)	65° (±1°)	See Fig. 2.
AGB.	Canterbury III	Kiln	A.D. 100-120	Good	12	1° E (±2°)	63° (±1°)	See Fig. 2.
AGC.	West Stow, Suffolk	Kiln	2nd cent. A.D.	Fair	4	10° W (±3°)	69° (±2°)	See Fig. 3.
AGD.	Wattisfield, Suffolk (Grundle Farm: lower level—see AA).	Kiln	Perhaps 13th- 17th cent. A.D.	Poor and perhaps disturbed	9	14° W (±3°)	66° (±1°)	See Fig. 2.
AGE.	Upper Sheringham, Norfolk	Kiln	A.D. 100-150	Good	4	84° E (±1°)	43° (±2°)	See p. 172.
AH.	Rickingham, Suffolk (Kiln Farm —see S).	Kiln		Poor	5	5° W (±3°)	70° (±2°)	See Fig. 3.
AI.	Rickingham, Suffolk	Kiln			7	0° (±5°) (Weakly magnetized and erratic)	60° (±2°)	See Fig. 2.
AJ.	Rickingham, Suffolk	Kiln	Later 1st cent. A.D.	Poor	4	Too weakly magnetized		
AK.	Canterbury IV	Kiln		Fair	10	13° W (±3°)	70° (±1°)	See Fig. 3.
AL.	Stoke-on-Trent (Trent Vale)	Kiln	A.D. 50-75	Good	6	9° W (±3°)	71° (±2°)	See Fig. 2.
AM.	Salmonby, Lincs (Holbeck Manor Farm)	Burnt area	Bronze Age	Poor	2	Erratic	70°	See p. 173

NOTES. U, X, Z, AB, and AD were collected by the excavators and sent to us.
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Our experience suggests that satisfactorily magnetized samples may be obtained from a large proportion of fired structures. Most of our work has been done on the walls of kilns. These were the structures most readily available to us and they have proved also the most convenient. But hearths and the remains of burnt buildings are useful too and can often be dated more exactly.

A study of this kind depends on the help of field archaeologists, and we hope that we shall continue to profit by it. Some, but not all, of our debts are noted at the foot of the Table. Prof. E. Thellier has been most generous with advice and aid. We owe much, too, to Dr K. M. Creer, who till 1954 was the principal collaborator on the geophysical side and was active in the collection and preparation of samples. Prof. S. K. Runcorn gave helpful counsel in the early stages. Mrs K. Cook has throughout taken part in collecting. Mr S. C. Collard has contributed in various ways, especially in the preparing of specimens; so too has Mr A. Reeves. The Managers of the Dame Mary Grainger Sandys Fund and of the Craven Fund and the Department of Scientific and Industrial Research have contributed towards the cost of the work. For all this and other assistance we express our warm gratitude.

P.S. According to their excavators AK is late 1st–2nd cent. A.D., and AF/AGA *c.* A.D. 200–230. These small corrections, received during printing, could not be used in the Figures or Table.

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Edward Lhwyd and the 1695 *Britannia*

by FRANK EMERY

Mr Emery is a South Welshman who was formerly Lecturer in Geography at University College, Swansea and is now Demonstrator at the School of Geography, Oxford—a post once held by the late Editor of ANTIQUITY. He is interested in the regional work undertaken in the 17th and 18th centuries by natural historians and antiquarians like Lhwyd and his contemporaries, and in this paper, commissioned by Dr Crawford when he met Mr Emery at the annual dinner of the Herbertson Society in Oxford last year, he discusses Lhwyd's methods of collecting information.

MANY regional studies were published in this country between 1660 and 1730. Their authors wrote for a well-informed, critical public among the gentry and clergy of their counties, where the study of antiquities and natural history flourished. The new edition of Camden's *Britannia* (1695), 'a fitting product of the age',¹ illustrates perfectly the way in which writers were tested by local field-work. Gibson assembled a team of eminent men to co-operate in revising the great work and to bring it up to date with additions of their own. Edward Lhwyd (1660–1709) was one of the most successful contributors, whose work with the Welsh counties Hearne thought 'the very best Part of all the Additions'.² How did Lhwyd set about this task?

Although Gibson recommended him to the publishers in May 1693, Lhwyd was originally brought into the *Britannia* circle by William Nicolson of Carlisle (1655–1727). The two men were close friends and corresponded from Lhwyd's appointment as Keeper of the Ashmolean Museum (1691) until his death. At the outset Nicolson asked for his help in explaining North Country river- and place-names, but his field archaeology there led Nicolson to natural history, especially the new study of fossils, and together they became 'my most beloved Recreation'. Their letters in 1692 and 1693 developed the theme of Cumbria and North Wales as 'much of a piece' in their geology, botany and early history. Nicolson was so satisfied with the results that early in February 1693 he wrote to Gibson asking 'that you (i.e. Lhwyd) might be prevailed with to undertake Wales in this new Edition of Camden'.³ Full of enthusiasm, Nicolson contrasted the original *Britannia*'s 'poor appearance' with 'what we now hope to see', and (since it needed emendations in every page) wished 'we may have them done now to the purpose'.⁴

To make his contribution Lhwyd built up a network of correspondents throughout Wales. In order to do this he used the strong regional connexion that existed between Jesus College, Oxford, and the Principality; at no time had the College been 'more

¹ D. C. Douglas, *English Scholars*, 1939, p. 334; for this edition see Stuart Piggott, 'William Camden and the *Britannia*', Reckitt Archaeological Lecture, British Academy, 1951, pp. 209–213.

² Richard Ellis, 'Some Incidents in the Life of Edward Lhwyd', *Transactions of the Honourable Society of Cymmrodorion*, 1906–7, p. 12.

³ Bodleian Library, MS. Ashmole 1816, fo. 468: Nicolson to Lhwyd, 16 February, 1693. Some of Lhwyd's correspondence is printed by R. T. Gunther, *The Life and Letters of Edward Lhwyd*, 1945.

⁴ *Ibid.*, fo. 464: 21 June, 1693. In this letter Nicolson also stated the need in England of a Society of Antiquaries similar to that in Sweden; this was a year before he wrote to Thoresby on the same topic (Douglas, *op. cit.*, p. 131).

interesting or more national'.⁵ He sought the help both of his contemporaries there and of older men who had returned to Wales as schoolmasters, doctors, or in the Church. His correspondents were essential to the speed, ease and confidence with which Lhwyd completed his additions for *Britannia*, and partly explain their excellence. Further, they encouraged Lhwyd to proceed with his own principal work in Wales, and later in the rest of Highland Britain. In themselves, too, they represented the host of antiquarians and natural historians then busily investigating the countryside. An important national undertaking can be viewed through their eyes, as they exercised a critical curiosity on local problems.

Lhwyd had several correspondents in South Wales, and it is proposed to consider the man who reported on Pembrokeshire and Carmarthenshire, to whom Lhwyd was indebted for some notable first-hand data. Nicholas Roberts was born at St David's in 1646. At nineteen he went up to Jesus College as a servitor *plebis filius*, became B.A. in 1668 and moved to Christ Church in 1670. He took Holy Orders and returned to no less than four livings in Wales: as Rector of Llanddewi Velfrey (1673) he spent most of his life there on the borders of Pembroke and Carmarthen. He secured the parishes of Nolton (1678) on St Bride's Bay, Llanddewi Aberarth (1687) on the Cardiganshire coast, and (as he began writing to Lhwyd) Llanfallteg, just across in Carmarthen from his rectory at Llanddewi. His letters show plainly how well he knew those south-western districts of Wales, especially his native Pembrokeshire. He heard of the proposal for a new *Britannia* in August 1693, 'a very needfull, as well as usefull undertaking, for . . . it is very full of mistakes, or oversights, in those parts yt. I know'.⁶ It was by no means the first *Britannia* that Roberts had worked for; twenty years earlier he contributed to John Ogilby's proposed new survey of the country, sending him 'some remarks either of things pass'd by or insignificated by Mr Speed & others' in Pembroke and Carmarthen.

Now there was a chance for reviving and enlarging those unused notes for the more certain proposals of Gibson. Roberts began at once by sending coins for the Ashmolean ('There were lately found in my neighborhood by herd boyes 2 potts full of Roman Coyns . . .').⁷ Perfectly willing to help Lhwyd, he insisted on knowing how quickly he had to put his material in order. Lhwyd then sent a long list of subjects to Roberts, who thought that to answer all of them 'would swell to a volume for each County, & would contain not only ye Antiquities, but ye modern and naturall history thereof'.⁸ Clearly Lhwyd's interpretation of the edition's scope was likely to cause consternation right from the start. Even so, Roberts completed and posted off six folio pages very closely written about Pembrokeshire. Then he turned to Carmarthenshire, where again he accepted field-work as a matter of course; whenever told of a site, he made it his business 'since I intend an historicall account of this County, to enquire & view ye place'. In the matter of maps, too, Roberts insisted on accuracy and repeatedly criticized Speed and Camden from his knowledge of their maps—'notoriously false', 'unsufferably erroneous as to ye Topography & Orthography of places'. (Lhwyd also had set ideas on the new maps for *Britannia*). Indeed Roberts believed the best way to ensure an exact edition was 'an actuall survey by some ingenious person yt. would have undertaken 2 or 3 of his neighboring Counties'.

Even before Roberts saw a copy of the new *Britannia* he agreed to help Lhwyd with his

⁵ Ellis, *art. cit.*, p. 3.

⁶ MS. Ashmole 1817a, fo. 307: to Lhwyd from Llanddewi Velfrey, 21 August, 1693.

⁷ This find is included in the list of 'Roman Coin Hoards in Wales', *Bulletin of the Board of Celtic Studies*, I, 1923, p. 352, where the reference is given as MS. Ashmole 1815 instead of 1817a.

⁸ MS. Ashmole 1817a, fo. 312: Roberts to Lhwyd, 11 December, 1693.

PLATE XVII



BARNENEZ SOUTH. (*Top*) Contractor's lorries engaged in carrying away the mound.
(*Bottom*) Section cut by contractor through Chambers B and C.

[See pp. 149-53]



(a)



(b)



(c)



(d)

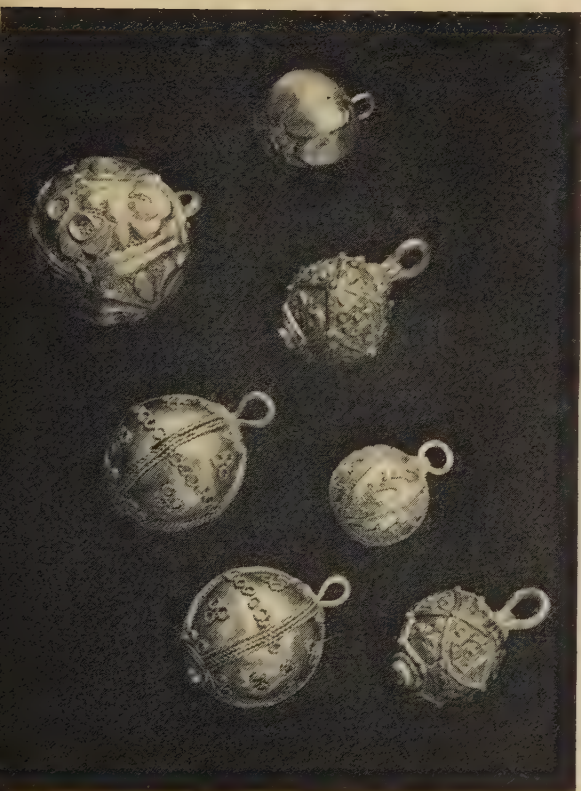
FINDS FROM MIKULČICE. (a, b, c) Silver Gilt Belt Ornaments. (d) Silver Crucifix. (a) and (b) : $2\frac{3}{4}$ in. long, (c) : $2\frac{1}{4}$ in. long ; (d) : 2 in. long.



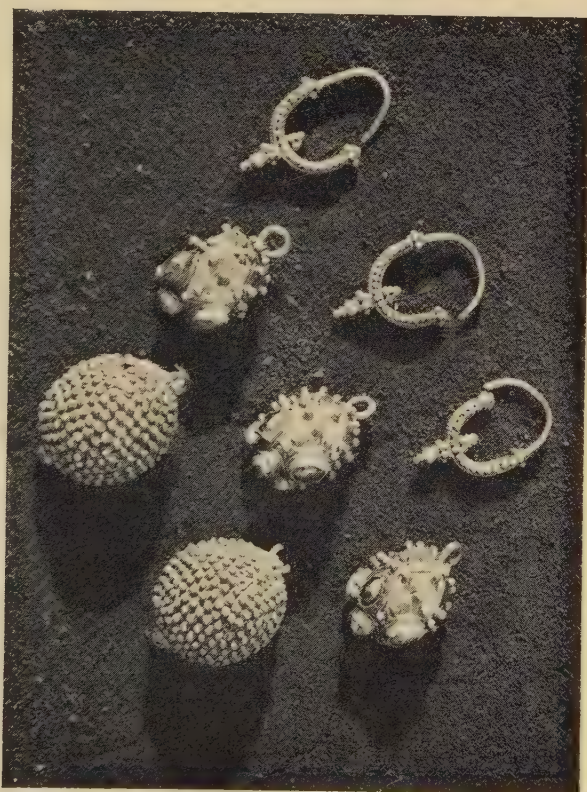
(a)



(b)



(c)



(d)

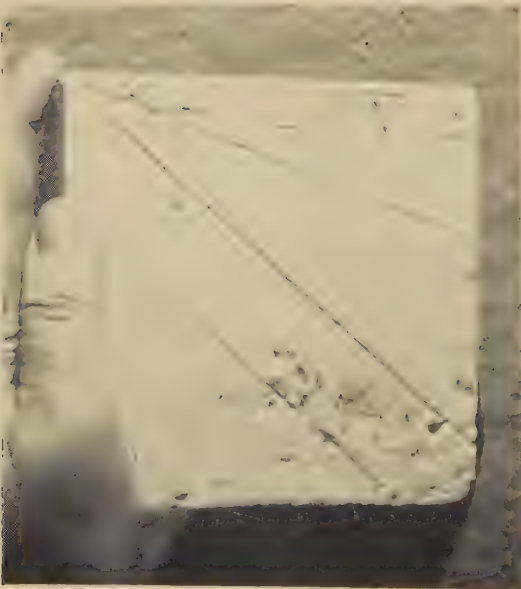
FINDS FROM MIKULČICE. (a) Gilt spurs ; 5 in. ; (b) antler roundel, 2 in. ; (c) and (d) gold ornaments, actual size.



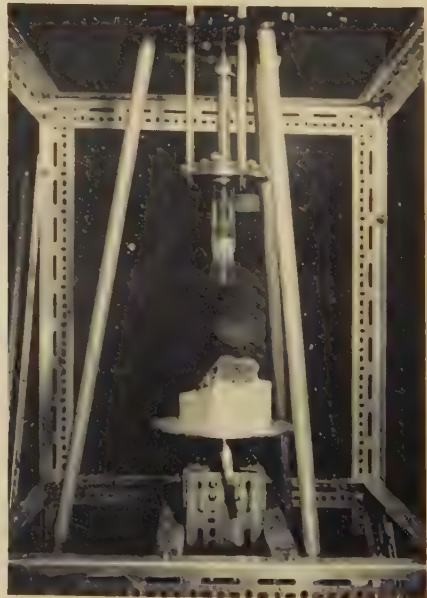
ARCHAEOMAGNETISM. (a) Dunnideer, vitrified fort. The samples have been cut round and enclosed in moulds.



(b) Filling the same moulds with plaster.



(c) Sample detached and freed from mould.
For full description see footnote 13, p. 172.



(d) Inner structure of magnetometer with sample on table.

plans for *A British Dictionary, Historical and Geographical, with an Essay entitl'd 'Archaeologia Britannica'*; and a *Natural History of Wales*.⁹ Roberts repeated his opinion of eighteen months earlier: 'It will be an insuperable Task to give so exact an account of each County in Wales, as Dr Plot has of Oxfordshire, unlesse you have many Correspondents, one for each 2 or 3 Counties at least.'¹⁰ It was true prophecy, for when Lhwyd died in 1709 this side of his project—the natural history—remained unwritten. For the moment, however, Roberts was a useful collaborator, if only because of his Carmarthenshire knowledge; this was not used in *Britannia* because of the publishers' refusal 'to swell ye Book to too great a Bulk'. His standards are implicit in his comments after receiving his copy of *Britannia*. At thirty shillings it was the dearest book he had ever bought, 'considering ye badnesse of ye paper, especially in ye Maps'. It is something of a surprise to read that Lhwyd had printed long extracts from the letters without Roberts's permission. 'Indeed I am not well satisfied with you . . . for had I understood so much I would have been a little more curious & inquisitive into particulars'.¹¹ Admittedly Lhwyd was in touch with many people in Wales and elsewhere, and was working at speed, but he was often careless or tactless both in his dealings with other *Britannia* correspondents and in his later work.

He certainly acknowledged the Pembrokeshire quotations (which take up nearly two columns of print) from 'my ingenious Friend, the Reverend Mr Nicholas Roberts', dealing mainly with natural history. Lhwyd believed the revised *Britannia* should include not only antiquities and civil history but such 'occurrences in Nature as seem more especially remarkable'. So he printed a paragraph on the migratory sea-birds Roberts had watched along the cliffed coast near St David's and on Ramsey Island; another on the blow-hole at Bosherton on the south Pembroke coast; and another on the making of black butter (or laver bread) from sea-weed. Bosherton Mere greatly interested Roberts and he gave Lhwyd far more detail about its working than appears in *Britannia*. He criticized several points in Lhwyd's additions, particularly for Carmarthenshire; e.g. as part of his field-work he examined Carreg Cennen Castle, whose caverns were described by Lhwyd and a remarkable spring noted by Camden. But to Roberts it seemed that 'neither are ye one copper mines, nor does ye other ebbe & flow. I could find no stone neer ye castle with any inscription, nor does it give name to a parish'. Travelling over most of Carmarthen, he 'personally surveyed many of its naturall & artificiall Curiosities' not mentioned in printed or manuscript books.

In dealing with antiquities Roberts made an interesting suggestion about one of the Welsh names for standing stones, which he knew on the Preseli hills. 'Meineu Gwyr are not, I suppose, so called from their crook'd or inclineing position, but from ye strength requir'd to bring them thither at first, being as it were manly stones, or stones of strength; for a weighty, heavy stone is very frequently called in our countrey Main gwrol'. Again, in the matter of an early Christian gravestone at Llandawke (Carms.) he examined 'a rude unpolish'd stone (formerly, as one then present told us, in ye Church), now one of ye steps under ye door';¹² he carefully copied its lettering, and records such as this were among the best results of field archaeology for the 1695 *Britannia*.

⁹ His proposals were published in November 1695, and can be read in Ellis, *art. cit.*, pp. 13–15.

¹⁰ MS. Ashmole 1817a, fo. 316: 3 August, 1695.

¹¹ *Ibid.*, fo. 319: 9 October, 1695.

¹² The stone, with Latin and Ogam inscriptions and now back inside the Church, is listed by V. E. Nash-Williams, *The Early Christian Monuments of Wales*, 1950, p. 113. Lhwyd's notes on sites or monuments are often invaluable evidence of their original plan or condition, e.g. his description of the stone circle at Llandyssilio, or the Paulinus stone from Cynwyl Gaeo (Carms.).

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Finally, their correspondence tells something of the course Lhwyd took in his 'Camden Tour' through Wales, the 'journey speedily quite through it' that so upset his publishers. No diary of his itinerary in 1693 has survived, so that (after leaving Oxford in mid-August) his progress is almost unknown. However, from his letters to the Ven. John Williams (1649-1701, Fellow of Jesus College from 1670 to 1681), his correspondent in Gower, it is certain he was at Swansea late in October 1693. Not far from the town on the slopes of Mynydd Gellionen he inspected a standing stone, which duly appeared in *Britannia*. Letters from Nicholas Roberts show that Lhwyd was at or near Llanddewi Velfrey in mid-December, so he had spent November travelling westwards into and about Carmarthen-shire (and, possibly, the southern Englishry of Pembroke). It seems certain that he spent at least four months in South Wales. He did not visit his Pembrokeshire correspondent, who naturally complained of this unkindness, 'yt. being in ye neighborhood you did not give me your company. For I could have given you a larger & better account by discourse in an hour, than by writing in a quire of paper.'

There is everything to be said for a closer investigation, perhaps carried out on a co-operative basis, of those men—especially the field-workers—who collaborated with Gibson in revising *Britannia*. Material is not lacking. For instance, a start can be made with Lhwyd and some of his mentors and correspondents; there are interesting records of Dr Robert Plot's itinerary in his native Kent during the summer of 1693, while Professor Eric Birley has drawn attention to William Nicolson's activities in Northumberland.¹³ It should also be possible to do likewise for Sir Robert Sibbald's researches in Scotland.

¹³ 'A Roman Altar from Staward Pele', *Archaeologia Aeliana*, xxviii, 1950, p. 144; and on Nicolson's Diaries, *Trans. Cumberland and Westmorland Antiq. and Arch. Society*, L (New Series), 1951, p. 112.

Danish Prehistory: A Review

by STANLEY THOMAS

Mr S. E. Thomas is the first holder of the newly created Lectureship in Archaeology at the University of Leicester. He has travelled extensively in Scandinavia and spent several years in Sweden engaged in prehistoric research. In this article he reviews two recent books on Danish prehistory, Land of the Tollund Man: the Prehistory and Archaeology of Denmark (Lutterworth Press, 1957; 160 pp., 30s.), by Palle Lauring, first published in Danish under the title of De byggede riget in 1954, and Denmark before the Vikings, by Ole Klindt-Jensen (Thames and Hudson, 1957; 146 pp. and 73 photographs, 21s.). All the illustrations in Lauring's book and all but four in Klindt-Jensen's work are by Lennart Larsen, photographer to the National Museum, Copenhagen and set a new standard in archaeological photography of which many other National museums will be jealous.

WITH the exception of Shetelig and Falk's *Scandinavian Archaeology* and a number of specialist studies, the prehistory of the northern European countries has not been readily accessible to the English-speaking public, and this has been most marked in the field of general interpretative studies. Three volumes have recently appeared which fill a large part of this deplorable gap—Geoffrey Bibby's *Testimony of the Spade*, in which the contribution of Denmark to archaeological theory and technique is enthusiastically, if somewhat naïvely, put into correct perspective, and now the two books on Danish prehistory up to the beginning of the Viking Age by Palle Lauring and Ole Klindt-Jensen. Primarily directed towards the lay enthusiast, they admirably complement one another to provide a fairly balanced historical and interpretative survey within their chronological and geographical limits. Since their readers will undoubtedly include many serious students and even professional archaeologists, however, their virtues and limitations both in general treatment and detailed approach invite comment on a higher level than would be justified for purely popular works.

As reference sources, it is astonishing that Lauring has no bibliography at all, failing even to mention Brøndsted's classic three-volume work, *Danmark's Oldtid*, and only one single reference within the text, to 'Mr' Clark's Star Carr publication, while Klindt-Jensen, to whom one would naturally look for bibliographical information, does include the major recent literature but excludes almost entirely that dealing with Denmark as part of the wider field of European prehistory as a whole.

The translations of the two books are of unequal virtue. *Land of the Tollund Man* is slightly stilted in style and traces of 'Danishisms' disturb the smooth reading of some passages; the text of *Denmark before the Vikings*, however, is excellent apart from the single misrendering of *Flække* as *flake* in the Mesolithic section—these are, of course, the *blade* cultures in standard terminology. The only typographical error of any moment is the confusion of dates (3000 and 2500 B.C.) in Lauring's chronological chart, while the misspelling of *Ertebølle* in Klindt-Jensen's chart might also cause slight worry.

The illustrations, which abound in both books, are admirably selected, and in fact are often identical, those in Klindt-Jensen being clearer and more brilliant. It is pleasing that, like so much illustrative material in recent Scandinavian literature, they are true pictorial

compositions and give a far more immediate impression of form, texture and artistic achievement than the flat photographs of standard professional literature, though the requirements of exact measurement necessitate the latter as a general rule.

In general content, Klindt-Jensen's study is easily the more valuable; it is well-balanced, scholarly and unobtrusively scientific, unmarred by unconsidered statements or unverified fact, and the detailed excavation reports and interpretative researches that comprise the structure of Danish prehistory lie close below the surface. In brief, his is a complete textbook, lacking only the footnotes, a limitation that may well be deplored by readers interested in the mechanics of interpretation. Lauring, who covers exactly the same ground, writes on a more popular level and in his attempt (praiseworthy in principle and by no means unsuccessful in the result) to bring the past to life, unfortunately allows himself too much corroborative detail which, despite his frequent protestations that this is guesswork, leaves a wholly unjustified impression of being established or even accepted fact. Although much of his text is open to criticism, it is nevertheless true that works such as this form a valuable counterbalance to the frequently overcautious, formally scientific, textbooks of material remains. In Scandinavia in particular, where the typological tradition of Montelius and Müller is an unconscionable long time finding its true place as the tool and not the final objective of the archaeologist, it is perhaps ungrateful to criticize excess zeal in a follower of Sven Nilsson or even Worsaae himself.

The immense wealth of Denmark in prehistoric material, and especially her Bronze Age brilliance, is not the only reason for her leading position in northern European prehistory. The survival of organic material in her peat bogs, from Mesolithic fishing nets to the Hjortspring and Nydam boats, enables a remarkably full picture to be drawn of material culture, daily activities and domestic crafts. The costumes from the Bronze Age oak coffin burials and the Iron Age finds from Thorsbjerg (the authors justifiably include parts of N. Germany and S. Sweden in their orbit when occasion demands) are of immense importance for the humanization of prehistoric cultures. The number and variety of votive finds, chiefly from peat bogs, provide a basis, however unstable for spiritual interpretation, and are supplemented by rock-engravings and other artistic representations. Burials and, by comparison with other areas, abundant, settlement remains, can be made to yield evidence of social structure and development, while the objects themselves provide the basis for material, economic and technological studies. It is thus partly due to a happy combination of cultural wealth in many periods and special survival conditions that Denmark has come to hold a special position in archaeology, and this is abundantly evident in these two books, even to the extent of provoking a rather nagging, nationalistic overtone in Lauring's case. What is not so evident from the modest claims of both authors, is the shining example set by the Danish countryman and professional archaeologist. The conscientious reporting of finds by an alert and archaeologically educated public, especially the farming community, the intensive field survey and meticulous excavation traditions of the professional and amateur archaeologist (unfortunately not well illustrated in the Plates), and not least the classic contributions of experts in many allied sciences, including such practical control experiments as the reconstructed slash-and-burn agriculture in S. Jutland, have all achieved such a degree of precision and excellence that Denmark excites the admiration and envy of archaeologists everywhere. These books could not be expected to record these achievements except by implication, but without them they could not have been written and their interpretative detail, which in fact is reliable inference, would be fantasy.

The two aspects of society most vividly illustrated by the Danish prehistoric material are its class structure and spiritual institutions and concepts. The social structure is most

carefully traced by Klindt-Jensen by extracting the full evidence from burials and, where possible, from settlement remains. The small groups of Mesolithic hunters can be seen to give way to village (enlarged family ?) communities in the compartmented long houses of Barkaer in the Early- and Troldebjerg in the Middle-Neolithic; the break in the evidence in Late Neolithic times apparently conceals a drastic change, implied by patent differences in economy and burial, while Early Bronze Age burials indicate the rise of a new and potentially significant social stratification based on wealth, contrasting with the persons permitted burial in megalithic tombs where no evidence for wealth exists. In the Early (pre-Roman) Iron Age a clear-cut division appears in the burials between pit- and urn-depositions with few, poor grave goods and the rich carriages of Kraghede and Langoå, but difficulties arise when one tries to relate this evidence to the substantial rectangular farmhouses of the period, presumably to be associated with the possessors of the poor burials on account of their frequency, but implying no such exaggerated social distinctions. The magnificent Roman period imports into Denmark, especially the Hoby cups, are undoubted evidence of a powerful and rich ruling class, and provide a link with the Dark Age social structure of chieftains (even kings), freemen and slaves from which Viking society grew.

The assessment of the pre-Roman and Roman Iron Age in Denmark presented here and in his *Foreign Influences in Denmark's Early Iron Age* is Klindt-Jensen's major contribution to Danish and European prehistory. Taken in conjunction with Wheeler's *Rome beyond the Imperial Frontiers* and the researches of Eggers and others on the Continent, his studies show the effect of Rome on the surrounding peoples of northern Europe and reveal the nature of the varied relationships usually concealed by the terms 'imports', 'affinities' and 'influences'. The interplay of cultural, diplomatic and commercial exchanges, the proceeds of loot, raiding and victory of arms, the dispersal of ex-servicemen's equipment, etc. provide a warning to students of earlier periods where evidence is more slender, literary sources are absent and culture associations obscure.

In the field of spiritual interpretation both authors have valuable contributions to make. Klindt-Jensen is restrained and factual, perhaps excessively so, while Lauring gives his imagination free rein and indulges in enthusiastic, sometimes brutal, but always stimulating theory. Both make frequent reference to Scandinavian folk-lore with interesting and pertinent results, both make full use of the literary sources, especially Tacitus and the saga material for the interpretation of Iron Age and Dark Age practices and deities, and both build on comparative ethnology, Klindt-Jensen under strict control and Lauring quite uninhibitedly. In the event, and despite a degree of detail which cannot possibly be accepted as reasonable inference, it is Lauring who brings home the essential point that not merely do the supernatural and natural meet in every act, every tool and every locality for primitive man, but that its spiritual concepts form the very life-blood of early society. Lauring attempts the interpretation of both the specific evidence for ritual practices and the broader aspects of the spiritual background of the various prehistoric economic phases. The ritual and magic objects in Danish archaeology are indeed of outstanding interest—the human figures incised on the bone object from Ryemarkgård, the specialized ritual associated with the Scandinavian megalithic sphere, the practice of trepanning, the Trundholm sun chariot, the *lurer* and figurines of dancing girls and horned gods, the abundant and evocative votive hoards—pottery, axes, metal objects, plaits of hair, wooden figures, etc.—from all periods, the bodies of Tollund Man and the Sleswig Girl, and above all the Gundestrup cauldron; here, if anywhere, the spiritual concepts of early man should come within our grasp.

In spite of this abundance of evidence for details of specific rites, it is still impossible to

comprehend the underlying beliefs which they mirror. Broad distinctions can perhaps be drawn between the hunting magic of the Mesolithic communities, the fertility basis of various aspects of Neolithic religion, the varied rites of a sun-cult in the Bronze Age and the personified concepts of the deities of the Iron Age developing into the complex beliefs of Viking society. Throughout the whole period runs the series of votive finds by lakes and rivers, etc., but how these different traditions are to be interrelated, how they arose and developed and how they compare with parallel phenomena in neighbouring regions must for the present, and perhaps for ever, remain a mystery.

Finally, these two books present a picture of the much neglected background to the Viking period, the subject of a forthcoming volume in the Ancient Peoples and Places Series by Holger Arbman of Lund. Too often the Vikings seem to represent a self-contained and disconnected cultural unit, while their less spectacular ancestors are lost in either the gloom of the Dark Age or the turbulence of the great Migrations. The last chapters of these two books should therefore be read as an essential introduction to all Viking studies (even though they are necessarily restricted to the Danish branch of the Vikings); only through a knowledge of their historical, economic and cultural background can the Vikings' achievements—and excesses—be properly understood.

Book Chronicle—*continued from p. 162*

EGYPTIAN YEARS. By L. A. TREGENZA. *pp.* xx + 198, 8 *plates*. Oxford University Press, 1958. 25s. A description of agriculture in the Thebaid where the old basin system of irrigation still survives, and of archaeological reconnaissance in the Eastern Desert, by the author of *The Red Sea Mountains of Egypt*.

THE EXCAVATIONS AT QUMRAN. By J. VAN DER PLOEG. *pp.* xi, 233, 15 *plates*. Longmans, Green, and Co., 1958. 16s. 6d. A translation of 'Vondsten in de Woestijn Van Juda' (Utrecht, 1957), in which the author, Professor of Old Testament and Hebrew at Nijmegen, surveys the Judaean Brotherhood and its Ideas.

ALBUM OF DATED LATIN INSCRIPTIONS. By ARTHUR E. GORDON in collaboration with JOYCE S. GORDON. *Two volumes. I. text pp.* 160; *II. 66 plates*. University of California Press, 1958. \$15.00. Part I. Rome and the Neighbourhood, Augustus to Nerva. Part II of this Album, now in preparation, will cover the Republican period and include inscriptions from Greece, Italy outside Rome, and other Mediterranean areas.

LACHISH IV. THE BRONZE AGE. By OLGA TUFNELL. *Two volumes. I. text pp.* 351; *II. 92 plates*. Oxford University Press, 1958. £8 8s. The fourth and last report on the late J. L. Starkey's excavations at Tell ed-Duweir, the probable site of Lachish, in south Palestine, sponsored by the Wellcome-Marston Archaeological Research Expedition to the Near East.

CHARLES DARWIN. By SIR GAVIN DE BEER. *pp.* 23. Oxford University Press, 1958. 3s. 6d. The 1958 British Academy Lecture on a Master Mind.

THE WORLD OF THE POLIS (*pp.* 389) and PLATO AND ARISTOTLE (*pp.* 382). By ERIC VOEGELIN. Oxford University Press for Louisiana State University Press. 48s. each volume. These are Volumes II and III of *Order and History*, described by the Yale Review as 'the most important historical work of our century', and written by the Boyd Professor of Government at Louisiana State University.

[*continued on p. 216.*]

Notes and News

JERICHO AND COMPOSITE SICKLES

Professor Carl O. Sauer, Professor of Geography at Berkeley, California writes:

Having been out of the country much of the past year I am catching up on the things I should have read: thus I have only made the belated discovery by reading *ANTIQUITY* (1956, 132; 184; and 1957, 73 and 82) that there has been great news out of a more ancient Jericho. We have also been underestimating elsewhere the age of cultural advances. In the New World we must not only admit a much earlier presence of man, but concede more time to textile skills, to agriculture, and probably also to metallurgy and to the directed employment of labour in mass. Perhaps this means that the great cultural changes which we like to call ages or periods have had long and varied antecedents.

The revision of prehistoric time is due largely to radiocarbon measurements. Their steady refinement and growing number of accordant results give increasing confidence that we now have something approaching a valid scale of time. Zeuner has noted at Jericho that some samples give a reduced age because of the presence of younger carbon. Such geographic conditions of subsequent additions of later carbon appear to be much more common than the opposite distortion. The question whether radioactive carbon has been formed at a constant rate has been under scrutiny from the beginning. Braidwood has raised no new reservation of validity; there is no reason for singling out the Jericho dates as suspect; if there was variation in rate it was not by geographic location of sites.

The great spring of Jericho, issuing from the base of the Judaeian plateau into the arid Jordan basin, must have been an obligatory place where men met and lived from earliest times. Here physical geography marked, about as precisely and enduringly as anywhere, the exact site where a town must be whenever the time came that men built towns. That the two oldest known towns should have been discovered here is less surprising than the fact that both were fortified towns. Miss Kenyon has considered possible meanings of the walled towns. The implications are rather in favour of the doctrine of original sin, that civilization was helped along by fear and enmity, for which this may be a test site. At any rate Miss Kenyon does not appear concerned about social theories, but about where to dig next and what studies to make of the remains of town living.

Archaeology is assembling a record of remote time and ways that brings better knowledge of culture origins and processes. It is working soundly and thoughtfully with its own evidence. Does it need the reorientation proposed by Braidwood that it learn 'the general philosophy of the social sciences' whatever that is, if there is such? That social theorists, on the other hand, should be eager students of culture history would appear a reasonable expectation, but they think quite otherwise.

In the United States some archaeologists are adjusting their inquiry to fit current anthropological notions of independent parallel evolution. This is pretty much a dusting off of the old theory of stages, garnished with terms that are doctrinal symbols. There has been for some time a vogue of 'culture areas', pretty much based on the premise of autochthonous development and giving minimal heed to intruding peoples and ideas. 'Diffusion' is not reputable at present as 'evolution' is. I think this is 'the conception of the area co-tradition by the Americanists' recommended by Braidwood. He sees the Near East as 'the scene of a single generalized area co-tradition, which has a more or less uniform development'. Its village farming had 'an as yet only vaguely conceptualized era of incipient agriculture before it. This *may* have had its own even vaguer area co-tradition, and this *may* even be what Sauer is talking about.' It is not what I was talking

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about and I can only guess vaguely that he is thinking of the Near East, as I do not, as one self-generated and self-contained development.

The Jericho discussion having raised again the matter of the serrate stone sickles and my comment thereon made in 1952, I need to reconsider their meaning. These implements are being reported from more sites, over a larger area, and perhaps over a larger span of time. They were an invention that gained wide acceptance and must have been considered significantly useful. At first I accepted their interpretation as a grass-cutting tool but doubted their use in the reaping of wild grass seeds because the seed would be lost by shattering. Since domestication involved selection to develop a rachis that would not shatter on ripening, I thought that the presence of such sickles argued for the cultivation of domestic grain. I no longer like the idea. My revised ideas have had the benefit of discussion with Hermann v. Wissmann and Karl Narr, but the responsibility for the following interpretation is my own.

Primitive harvesting of grass seeds, wild or cultivated, makes use of one of several methods, each simple, efficient in saving seed, and adapted to the kind of plant harvested. In the New World the most common procedure is to beat or brush the seeds into a tray or shallow basket. The stick used may be flattened and thinned to an edge, roughly a wooden blade; the receptacle is broad. Or, the seeds may be stripped by hand. Stiff-stalked sorghums are gathered by breaking off the panicles. Where annuals are grown in mixed plantings, as is still done in the Deccan of India where various grains and pulses are sown together, the whole plant is pulled up by the roots. The plants are gathered singly as they become ripe or are wanted.

In undisturbed natural situations the plants of interest to man mainly grew scattered through a diverse vegetation, annual and perennial, herbaceous and woody. Harvesting, as I see it, was of individual plants and small colonies. The primitive forms of cultivation known to me also are mixed plantings. I doubt that the idea of segregating plants, each kind to its own field, occurred to cultivators, or made sense, until plough, harrow, and draught animals were introduced. Cultivation, I take it, was carried on in the Near East long before there was plough agriculture. Under mixed seeding plants matured at different times, there was repeated selective harvesting of the planted ground, and no use for an implement of mass reaping that would leave a field of stubble.

The composite sickles are ill suited for reaping sheaves of grain, wild or domestic. I think they would work well to strip seed heads by a raking pull into a container. In such case the serration is an improvement on the blade of the seed beater. However, the improvement seems an excessive expenditure of labour and skill for the end attained. I refer to the pointing of the flints, their matching, and the care in securing them in the blade. They may have been thus used, but I doubt that they were invented in order to rake off seeds.

The saw-toothed invention makes sense as archetype of the saw. With it man was better able to secure the raw plant materials for building, roofing, house-furnishing and personal equipment. The teeth, drawn back and forth, severed tough stalks and leaves hard to pull or break off but useful because they had strong and long fibres. We know of no people too primitive to make some use of plant fibres. Specialized skills in working with fibre plants were developed very long ago. In the latest years caves and rock shelters in Oregon, Nevada, and Utah have yielded a good many well-made fibre products, such as sandals, mats, baskets, braided and twisted cords, and nets. The artifacts chanced to be preserved because of the dryness of the site. They were made by people who lived near freshwater bodies during the last Pluvial time of the Great Basin, and in part are older by some thousands of years than the earliest known record of Jericho. The fortunate accident of

preservation gives a different and more advanced cultural picture than would have been construed from the simple and few artifacts of stone. (Sauer, 'The End of the Ice Age and Its Witnesses,' *The Geographical Review*, vol. XLVII, no. 1, 1957.) Kind and level of the old Jericho cultures justify the inference that they also made free and particular use of fibre plants, even without the discovery by Miss Kenyon that they 'had rush mats on the floors, of the same weave as those in use today'.

The spring of Jericho (which may have been even greater before overgrazing and other exploitation by man impoverished the plant cover of the upland) flowed out into marginal marsh, native habitat of such plants as bulrushes, cattails, sedges, canes, and osiers. In native villages of our arid lands, as in Mexico and Peru, a freshwater marsh may be valued as highly for such plants and managed about as well as is the tilled land; the situation at Jericho is suggested as similar. The nearby Judean upland supported tough perennial bunch grasses. Although the esparto grasses are north African and Iberian, there are local *Stipae* and similar siliceous tussock grasses, and also leguminous shrubs ('brooms' such as *Cytisus* and *Genista*). A study of the locally available fibre plants should be rewarding.

I offer these remarks as 'an exercise in imagination' not 'completely untestable'.

CARL O. SAUER

THE SECONDARY IRON AGE IN BRITAIN

The division of the British Iron Age into A, B, and C has proved its value over more than a quarter of a century.¹ But in the highland zone it is often difficult to fit this classification satisfactorily to the known remains. This has been recognized for some time by accepting the presence of a Late Bronze Age Survival culture outside the areas where there is satisfactory evidence for Iron Age A, B, or C. This name gives no indication that the people concerned had adopted the use of iron, and has the additional disadvantage that it is very cumbersome. Perhaps for this reason, very little attention has been paid to the nature of their culture; indeed, there seems to be a tendency to spread the A and B culture-areas as far as the most meagre scraps of evidence permit, or even further.

The object of this note is to urge that the system of nomenclature now accepted for the Neolithic period should be applied to the Iron Age also: that is, the invasive A, B and C cultures which introduced the use of iron should be described as representatives of the Primary Iron Age, while the cultures developed by the descendants of the Late Bronze Age inhabitants who had adopted the full use of iron should be grouped under the title of Secondary Iron Age.

Although it is not the writer's intention to attempt to describe any of the Secondary cultures in detail, it seems desirable to indicate what can be recognized or suspected at present. Only one is clearly defined. On the eve of the Roman conquest much of the eastern part of Britain, from the Forth as far south as Yorkshire, was occupied by people using an exceptionally coarse type of pottery. This was first regarded as 'Votadinian'² but has since been found in quantity at Stanwick.³ In view of its distribution and character, Border ware would seem an appropriate designation, but in spite of its crudity it continued in use after a partial occupation of the territory by an offshoot of the Primary invaders from southern Britain,⁴ and even after the Roman conquest. The palisaded enclosures found in Northumberland and Roxburghshire⁵ are probably the dwellings of the Border ware users. It must be emphasized that this crude pottery cannot safely be used as evidence by itself for cultural connections, except where its distribution is fairly continuous: the remarkable Danish site of Trelleborg (c. A.D. 1000) has produced pottery which seems from the published report to be very similar indeed.⁶

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Another Secondary Iron Age group may be suspected in Caernarvonshire, where the hill-fort defences differ markedly from those known to be associated with Primary Iron Age peoples. There is, however, practically no other cultural material to supplement this evidence. A similar but not identical group probably existed in Pembrokeshire.

It is unlikely, though, that these few examples represent the true position. It is hardly to be expected that Secondary Iron Age peoples will be located when their existence is not recognized. So far as present evidence goes, it would seem that the Iron Age A invasions took place initially at several widely separated points,⁷ so that presumably pockets of the earlier (L.B.A.) inhabitants were left undisturbed for several generations, subject to influences from the Primary invaders but not conquered by them. In such areas further research should make it possible to detect the development of Secondary groups, even though they were later overrun by Primary settlers.

A. H. A. HOGG

¹ C. F. C. Hawkes; *ANTIQUITY* v (1931), p. 60; *Congresos Internac. de Ciencias Prehist., Actas de la IV Sesión, Madrid 1954*, (Zaragoza, 1956), p. 729.

² Professor I. A. Richmond, *Arch. Aeliana*, ser. 4, vol. xx, pp. 121-132. A. H. A. Hogg, in *Aspects of Archaeology* (London, 1951), pp. 214-9.

³ Sir Mortimer Wheeler, *The Stanwick Fortifications*, Soc. of Antiquaries Research Report No. XVII (1954), pp. 38-44.

⁴ C. M. Piggott, *P. S. A. Scot.*, 1949-50, pp. 131-4.

⁵ *Arch. Aeliana*, ser. 4, vol. xxxiv, p. 152; *P. S. A. Scot.*, 1948-9, pp. 64-7.

⁶ P. Norlund, *Trelleborg, Nordiske Fortidsminder*, iv, i (1948).

⁷ Dr K. M. Kenyon, *8th Annual Report of Inst. of Archaeology* (Univ. of London), pp. 29-78.

THE PREHISTORIC SOCIETY

Congratulations to the Prehistoric Society, which celebrates its fiftieth birthday in October of this year. It began, according to the official records, on 26 October, 1908, at a meeting in the Norfolk and Norwich Library, 'an inaugural meeting of an East Anglian Society for the study of all matters appertaining to prehistoric man'. On 7 December, 1908, the preliminary Committee appointed to draw up a constitution for the Society presented their Report but 'in view of the large number of implements on exhibition, consideration thereof was adjourned'. Apparently the Society was at first called 'The East Anglian Society of Prehistorians' for on 25 January, 1909, when the constitution of the Society was formally adopted, it was decided to change the name to 'The Prehistoric Society of East Anglia'. Seventy-two favourable replies had been received to the first circular suggesting the formation of the Society; when the first volume of the *Proceedings* was published in 1911 over a hundred names are listed as members. These included W. G. Clarke (one of the two Honorary Secretaries), Prince Duleep Singh, Baron von Hugel, Philip Laver, Miss Layard, J. Reid Moir, S. Hazzledine Warren and H. B. Woodward. The first President was Dr W. Allen Sturge, and in his Presidential address he said: 'It is high time that here in England we should begin to draw together, in one district at any rate, those who are interesting themselves in the absorbing but difficult study of the early ages of the human race before the dawn of history'.

The East Anglians were not the first group of people to found a Prehistoric Society. The Société Préhistorique de France had been founded in 1904 under the presidency of Emile Rivière, with seventy-four founder members, including Marcel Baudouin, Cazalis de Fondouce, Chantre, Paul du Chatellier, Léon Coutil, Daleau, Hanotiaux, Dr Henri-Martin, Adrien and Paul de Mortillet, Piette, Raymond—a more distinguished and more internationally known group than the East Anglians—but then France was the country

of prehistorians in the first decade of the 20th century, and the East Anglian Society began as a modest group of regional archaeologists. The English Society, however, soon realized that prehistory could not be best served by restricting it to one part of Britain and began to print on its *Proceedings* a notice that its subject matter was not restricted to the district from which it was named.

At the Annual General Meeting in Norwich Castle Museum on 23 February, 1935, Dr Grahame Clark, who had become Editor of the Society in 1933, and who this year assumed its presidency, moved 'That the title of the Society shall be *The Prehistoric Society*'. It then had a membership of 353. Today, as it celebrates its jubilee, the membership is approximately 1,100, including a considerable number from overseas. (Applications for membership to Miss J. M. Bull, 11a Kensington High Church Street, London, W.8.)

Apart from its regular meetings and the publication of its *Proceedings*, which draws material from all over the world and is one of the most worthwhile and best produced archaeological journals in Europe, the Prehistoric Society has in the last few years held short conferences in London and meetings in the field. The next London conference will be held at the Institute of Archaeology of London University during the spring of 1959, and the next field meeting will be held at Bangor between 29 August and 2 September, 1959. This year a field meeting was held at Easter in West Cornwall, and a correspondent sends us this account of it.

Meeting in Penzance on 10 April, the conference was opened by Prof. J. G. D. Clark, the new President, and Mr C. A. R. Radford, the retiring President, gave the introductory lecture. He spoke on recent archaeological work in Cornwall, stressing particularly the dominant pattern of the Iron Age, which can now be seen to fall into three main phases: an apparently brief and derived Iron Age A, followed by the almost classical South-Western B, which develops into the Cordoned Ware of, typically, the 1st and 2nd centuries A.D. The following morning the delegates met to hear a series of short talks. Mr B. Wailes spoke on the excavation of Sperris Cromlech, and the Penwith Chambered Tomb Group as a whole, suggesting that it should be dated to the Early Bronze Age. Mr A. C. Thomas, the Local Secretary of the Conference, gave a brief outline of the archaeological sequence at Gwithian, where he has inaugurated and organized, under the West Cornwall Field Club, a programme of intensive field-work and excavation in a small area: this has yielded a series of sites which give an almost complete record from the Mesolithic to the present day. The excavations of the Lizard Field Club at Kynance, an Iron Age settlement overlying a Middle and Late Bronze Age occupation, were then described by Mr J. I. Thomas, and the last lecture, by Mrs E. V. Clark, concerned the recently excavated fogou (souterrain) at Boleigh, which has at one side of the entrance a carving, apparently a figure with upraised arms. That afternoon the lately reorganized Museum of the Royal Institution of Cornwall at Truro was visited.

The last two days were spent on excursions round Penwith. On the moors above Zennor the Zennor and Sperris Chambered Tombs were visited, and Miss D. Dudley gave a short lecture at the Iron Age village at Sperris, excavated by the West Cornwall Field Club last summer. The tour continued via the two Scillonian Entrance Graves at Treen, the Romano-British Courtyard House site at Porthmeor, Chun Iron Age Hill-Fort and Chambered Tomb to the strange multiple cairn of Carn Gluze, apparently of Late Bronze Age date. The last stop was the unexcavated Cliff-Fort of Treryn Dinas, peculiar in having two widely-spaced single ramparts, with triple ramparts between them. On the last day, following visits to the fogou at Boleigh and the stone circle of Dawns Men close by, the excursion went to Gwithian, where the sixth season of excavation was in progress. Here delegates were shown round the sites in the area, perhaps the most important being

an occupation site giving a continuous stratified sequence from the Neolithic to the Late Bronze Age, and containing traces of Middle Bronze Age cross-ploughing, and a similar site nearby, occupied c. A.D. 400-1000, the first complete sequence for West Britain in this period. The tour ended at Carn Brea, which Mr Radford emphasized was an Iron Age Hill-Fort, the well-known Neolithic occupation being unconnected with the visible huts and ramparts.

INDIAN ARCHAEOLOGY

When Sir John Marshall assumed charge of the Archaeological Survey of India he inaugurated the series of *Annual Reports* of the Survey. In 1944 when Sir Mortimer Wheeler took over the Director Generalship of the Survey he felt that essential research matter should be published in a more attractive form than the *Annual Reports* and founded *Ancient India* as a Bulletin of the Archaeological Survey of India to contain individual articles relating to the archaeology of India and adjacent lands. The first issue appeared in January 1946 and under his vigorous editorship the first few issues soon established *Ancient India* as a major archaeological journal; they contained *inter alia* Prof. Piggott's article on 'The Chronology of Prehistoric North-Western India', the report on the excavations at Arikamedu by Wheeler, Ghosh and Krishna Deva, and the now classic comments on 'Recording of Archaeological Strata' by Wheeler and Piggott.

Ancient India continues and maintains the high standards of publication which it set itself (and others) over ten years ago. Shri Ghosh, the new Director General of Archaeology in India felt that in addition to *Ancient India* a new series of publications of his department was necessary giving an annual summary of the department's work in excavation, preservation, discovery, publication, museum development and training. The result was *Indian Archaeology* 1953-4: *A Review*, and the fourth volume in this series, for 1956-7 is now to hand (price 9s. 6d. or rupees 6 from the Department of Archaeology, Government of India, New Delhi). *Indian Archaeology* is well printed and illustrated and is extremely good value; the present volume has 85 pages of text including nineteen text figures and forty plates, two of them (illustrating the report on archaeological chemistry) in colour.

Indian Archaeology will be indispensable to all concerned with keeping abreast of the discoveries of prehistoric and protohistoric India. It will not be invidious to single out two extremely important advances in learning recorded in these four volumes. The first is the deepening of our knowledge of the Harappan civilization by the discovery and excavation of sites such as Rangpur and Lothal south of Ahmadabad in Kathiawar, and the linking of the Harappan civilization with later historical times at Rupar and Rangpur. Excavations at Rupar, sixty miles north of Ambala not only revealed a new Harappan cemetery but showed 'an almost continuous sequence of occupations from the Harappa to the medieval times, thus linking the protohistoric with historical archaeology' (*Indian Archaeology*, 1953-4, 6). Excavations at Rangpur showed that it 'had been a Harappa settlement with a long life' and that 'it provided for the first time a continuous cultural-sequence from the Harappa to the period prior to the Northern Black Polished Ware with hardly any break. Unlike Harappa and Mohenjo-daro, the Harappa culture here died a natural death; for it gradually deteriorated and transformed itself into a subsequent culture, characterized by the use of a lustrous red pottery' (*Indian Archaeology*, 1954-5, 11-12). The map of the Harappa Culture in Western India (*Indian Archaeology*, 1954-5, fig. 7, p. 60) shows the extent of these important discoveries and the possible routes from the mouth of the Indus to Harappan ports in the Gulfs of Cutch and Cambay.

The second major discovery recorded in the pages of these four volumes is that made

at sites like Nevasa, on the Pravara, a tributary of the Godavari, and at Prakash, on the Tapi. 'Next to nothing was known about the protohistoric and early historical archaeology of central India and the northern Deccan only five years back' says Dr Ghosh in *Indian Archaeology*, 1954-5. 'Persistent field-work has now brought to light a distinct sequence of cultures in this region from a chalcolithic age . . . down to the period when the Northern Black Polished Ware came into vogue'.

All success to this fine venture and to the work of the Department of Archaeology in India which it records and reflects.

RADIO-CARBON DATING AND ARCHAEOLOGY

Sir Mortimer Wheeler has recently reminded us (*ANTIQUITY*, 1958, 4) of how Lord Cherwell told him and Dr Crawford of the C 14 method of dating, and how this scoop made the *ANTIQUITY* editorial for September 1949 which began with the words, 'A discovery has been made in America which may be of the greatest use to archaeologists'. That discovery had first been made public in England in *Science Today* in March 1949, but the first archaeological radio-carbon date was published in a letter from Dr G. H. S. Bushnell (*ANTIQUITY*, 1949, 229). Nearly a decade has passed since the discovery by Prof. Willard Libby and his colleagues at the Institute for Nuclear Studies in the University of Chicago of the principle of radio-carbon dating, and these years have seen the development in America and Europe of special plants to produce the dates, and the determination and publication of results which amply justify the claim that the technique is 'of the greatest use to archaeologists'.

In England the first C 14 dates came from the plant in the Davy-Faraday Laboratory of the Royal Institution started six years ago in collaboration with Prof. F. E. Zeuner of the University of London Institute of Archaeology. This plant was a research project designed to come to an end in the autumn of 1958, and dating work at the Royal Institution is thus finishing. Meanwhile C 14 plants have been established in the British Museum, in the Sub-Department of Quaternary Research at Cambridge, and one is in process of construction at the National Physical Laboratory. The first results from the Cambridge plant, together with a description of its installation and working are published in a paper entitled 'Radio-Carbon Dating and Post-Glacial Vegetational History: Scaleby Moss' by H. Godwin, D. Walker and E. H. Willis in *Proceedings of the Royal Society*, B, volume 147, 1957, 352. And now there comes news of a new C 14 plant set up in Trinity College, Dublin, using a new technique of methanol in a liquid scintillation counter.

But, together with these developments, the last few years have produced some archaeological doubters, and some who, if not Carbon 14 agnostics, have wondered where this scientific dating is taking historians. In the first place there seemed to be no single and certain channel for the publication of radio-carbon dates, and even the most assiduous compiler of lists published in *Science* and elsewhere would meet someone who had just heard another new Carbon 14 date or had a gestetnered sheet of newer dates. Secondly, while many believed the physical principles to be sound, they began to wonder whether individual performance in separate laboratories all over the world was uniform. And lastly some dates began to appear which seemed to upset very considerably the current archaeological chronology built up, admittedly on exiguous data and unreliable premises, by pre-Carbon 14 techniques. Nowhere were these dates more surprising than in the Neolithic of Jericho and the Danube. It is not surprising that many archaeologists have been asking, 'is all well with this wonderful new technique?'

The first objection has now been met. At a conference on Radio-Carbon Dating held at

Andover, Massachusetts in October 1956 there was formed a Committee for Distribution of Radio-Carbon Dates, and this committee has recently sent round circulars about the service organization it has now achieved. It is producing a complete description of all dated samples 'on punched cards coded for sorting into universally useful categories'. These will be distributed to subscribers in the form of a 'unique set of cards having complete information otherwise next to impossible to assemble'. The intention is to distribute to each subscriber 5,000 cards during the next five years including 3,000 already available. The necessary coding and sorting equipment and index guides will be included and we are told that 'using your own codes you may add more than 79,000 categories specifically related to your research'. All this is breath-taking. Is chronology in prehistory now going to be reduced to a card index? Shall we soon be at a time when, instead of arguing typologies and synchronisms, we merely insert our sorting needle in pre-notched cards 'in one simple motion', as the committee describes it? However that may be, it is clear that no reputable institution concerned with the advancement of archaeology by teaching or research can afford to be without these cards. The address of the Committee is the R. S. Peabody Foundation, Box 71, Andover, Massachusetts. The total cost of these 5,000 cards (if enough people subscribe) is 250 American dollars.

All the dates in prehistory for £100 seems cheap at the price, but will all the dates be right? The doubts of some have been heightened by several recent pronouncements. In *Nature* for 1 December, 1956, Walter Elsasser of Utah together with E. P. Ney and J. R. Winckler of Minnesota referred to a possible source of error, namely the variation in the intensity of cosmic-ray formation, and this was based on the work of the Thelliers on remanent magnetism referred to elsewhere in this journal (p. 167). Prof. Zeuner has referred to the possibility that the carbon content of a sample can be washed out by alkalis with the result that the Carbon 14 age is higher than it should be. And in a recent article entitled 'Zur Anwendbarkeit der C 14—Datierung in der Vorgeschichtsforschung' (*Germania*, 1957, 102-10), Milošević subjects the whole method to searching criticism and concludes, 'a very accurate re-examination and over-hauling is absolutely necessary'. Many of these difficulties will be being discussed at Hamburg when this number of *ANTIQUITY* is published; meanwhile Mr Barker, who is in charge of the British Museum C 14 laboratory, has agreed to write for *ANTIQUITY* a comment on the criticisms of the method made by Elsasser, Zeuner, Milošević and others.

And new dates pour in as we write. Tom Harrison reports (*Nature*, 1958, 792) that in the Niah Great Cave in West Borneo charcoal from 100 in. down gave, according to Prof. de Vries's laboratory at the University of Groningen, a date of $39,600 \pm 1,000$ years, and that the tools occurring between this level and a previous Carbon 14 date of $32,630 \pm 700$ (*Man.*, 1957, 211) are uniface choppers with hammer marks on the butt which closely parallel some from the Soan of north-west India. Charcoal from the large wall constructions hitherto of uncertain date at Le Hague-Dike, Beaumont, in the Cotentin peninsula, give dates of $2,855 \pm 75$ and $2,710 \pm 65$ (H. G. Ostlund, *Science*, 1957, 493): these earthworks, thought by some to belong to the Viking conquest of Normandy, are Early Iron Age in date. And M. Giot has recently published in the *Bulletin de la Société Préhistorique Française* a Carbon 14 date for the wood from the floor of a dagger-grave at Kervingar, Plouarzel in Finistère: it is $1,350 \pm 50$ years B.C. With these and other dates coming in daily, the chronological exactness of the prehistoric record is almost outpacing the prehistoric chronologists. When we all have our cards and our needles, a silent revolution in prehistory will have taken place.

TRAVEL SCHOLARSHIP IN MEDITERRANEAN ARCHAEOLOGY

The Society for Hellenic Travel is sponsoring an archaeological cruise in the Mediterranean in the first fortnight of April 1959. It will visit Monaco, Filitosa in Corsica, Pompeii and Herculaneum, Piazza Armerina, Syracuse, Mistra, Crete, Mycenae, Tiryns, the Cyclades, Athens, Ravenna and Venice; and is being planned by Prof. Stuart Piggott, Lord William Taylour, Mr Michael Gough, and Dr Glyn Daniel, all of whom will accompany the cruise as guide-lecturers. The Society is offering one free place for a student member interested in Mediterranean archaeology and this will take the form of a scholarship providing free travel, accommodation and all meals and excursions from London back to London. The scholarship is intended for men and women between the ages of 16 and 21 years who are in the sixth forms of schools, the first year of their University studies, or suspended between school and University for reasons of military or other service. It will be awarded on the basis of an essay submitted by 15 January, 1959, which should be not less than 3,000 words in length and not more than 7,000 words and should deal with any aspect of Mediterranean archaeology. The essay should be typewritten (or written in a clear or legible hand) and may be illustrated with photographs, diagrams and drawings. It should not have been published previously and should be accompanied by a certificate from a schoolmaster or tutor attesting that it is the bona fide work of the entrant. The successful candidate or two or more candidates who have been short-listed, may be invited to attend an interview in London. If the successful candidate is unable to go on the spring cruise he or she may transfer the free place to a later cruise sponsored in the summer of 1959 by the Society for Hellenic Travel.

The subscription to the Society for full time students is 5s. a year, and full details may be obtained from the Assistant Honorary Secretary, Miss M. W. McCall, 4 Argyll Mansions, Chichele Road, London, N.W.2, to whom information of intention of competing for this Travel Scholarship should be sent by 15 November, 1958, and to whom the essays should be sent. Miss McCall will be happy to resolve any difficulties or obscurities in this scheme (which, if successful, the Society proposes to make an annual event), and her decision, in conjunction with the President of the Society, and Dr Daniel, a Vice-President, will be binding in any interpretation of it. The essays will be judged by a panel of archaeologists drawn from the officials and guest-lecturers, and their decision will be final.

THE PITT-RIVERS MUSEUM, FARNHAM, DORSET

The Pitt-Rivers Museum was founded in 1880 by Colonel Augustus Henry Lane Fox, F.R.S. (born 1827), who, in that year, inherited the estates of his great-uncle, George Pitt, second baron Rivers, and assumed the name of Pitt-Rivers. Since 1927 the Museum has been maintained and extended by General Pitt-Rivers's grandson, Mr George Pitt-Rivers. The Museum has been closed since the death, at the end of last year, of its curator, Major Joyce, whose kindness to visitors so many will remember. The re-organized Museum was opened on 1 July; much of the work of re-organization has fallen on Mrs Joyce, now the resident custodian since her husband's death. Of the re-opening and re-organization, Mr Hugh de S. Shortt writes:

The re-opening of the Pitt-Rivers Museum, on 1 July, was marked by something of the liberal hospitality of the man whose rich and varied collections are housed in this converted Victorian school of an out-of-the-way Dorset village. A very good luncheon was provided for a great number of guests by Captain Pitt-Rivers, the present owner, and coaches had been arranged to take London visitors from and to their trains.

Those who knew the museum well may have felt some qualms about the changes which might have taken place since the beginning of the year, but if so, a walk round the galleries

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might have re-assured them. There has been no major upheaval; the system of illustrating British archaeology by the ethnography of other races is still there, and so are General Pitt-Rivers's superb models of his own excavations. There has been a judicious thinning of the ceramic collections and certain other over-crowded exhibits, but in this there can be no complaint, and indeed it is a policy which might be pursued as time allows in some of the remaining galleries.

In an opening, or should one say a re-opening address, Captain Pitt-Rivers outlined the origins and aims of the museum, one of the very few private museums that remain. He explained how the principles of typology, which first led his grandfather to the study of archaeology, were demonstrated in the museum. His discourse was at times abstruse but never lacked wit and humour. He ended with a plea, which was echoed by an appreciative audience, for a more generous government policy towards an institution maintained for the public good, at private expense, since its foundation.

Sir Mortimer Wheeler also spoke with wit, and with a deeply-felt admiration for a museum which embodied not only the labour, but also the personality, of its distinguished founder. He strongly defended the existence in a Dorset village of an outstanding collection of Benin bronzes, and he might have added Migration period jewellery or half a dozen other collections, which it is so much more exciting to find here than in a metropolis. Only the presence of a Purbeck marble abbot from Cerne Abbas might cause those versed in the principles of Diocesan Advisory Committees to raise an eyebrow. Sir Mortimer gave a warning against too much help from the Treasury, as such help was unlikely to come without attached strings, and he hoped that Captain Pitt-Rivers would carry on as long as he could without it. One had visions of a man with a life sentence being urged to serve as much as he could.

Sir Leonard Woolley, Sir Thomas Kendrick, Dr Harden and many other distinguished personalities of the archaeological and museum worlds were present, but the one living link with General Pitt-Rivers, the man who knew him and worked with him, Mr H. St George Gray, was not there. At the start of the ceremony a telegram announced that he and Mrs Gray had been involved in an accident and, though not seriously hurt, had to return to Martock. We wish them, the Pitt-Rivers Museum and those who work for it, many prosperous years.

THE UNIVERSITY OF LONDON: INSTITUTE OF ARCHAEOLOGY

The Institute of Archaeology of London, founded in 1937 by Sir Mortimer Wheeler, has been an accepted feature in the archaeological scene since that time and the comparative remoteness of its setting in Regent's Park did not prevent it from playing an active part in archaeology before and after the Second World War, first under Sir Mortimer, then under the late Prof. Gordon Childe.

The Institute is now established within the precinct of London University on the north side of Gordon Square (the actual address is 31-34, Gordon Square, W.C.1; telephone no. Euston 6052) within five minutes' walk of the Senate House, in a new building which was completed early in the year and was officially opened by Her Majesty Queen Elizabeth the Queen Mother, Chancellor of the University, on 29 April. Prof. W. F. Grimes, who succeeded Prof. Childe as Director of the Institute in October 1957, writes:

The Institute of Archaeology shares its new building with the Institute of Classical Studies (Director, Prof. E. G. Turner) which, with the Roman Society and the Hellenic Society, has also transferred from its quarters in Bedford Square. This close proximity of several organizations with similar or related interests should be beneficial both to the organizations themselves and to their users.

The new building immeasurably increases the space available for the Institute's teaching and research activities. A feature of the Institute's work has always been its attention to the teaching of archaeological techniques, carried out as it was at Regent's Park in increasingly restricted conditions. The technical laboratories for the teaching of conservation take up part of the top (sixth) floor of the building: they consist of a small laboratory, a large restoration laboratory for pottery restoration and similar work, and a fully equipped metal laboratory. The photographic department, on the fourth floor, is laid out to facilitate practical teaching; it incorporates several dark rooms for students. The Department of Environmental Archaeology on the third floor is also fully equipped with laboratories and workrooms which provide for instruction and research in the natural sciences in their special bearing upon man and his activities.

The teaching departments are provided with seminar rooms fitted out with projection apparatus and with storage accommodation for teaching and research collections. The collection-storage arrangements are indeed a feature of the building. A system of standard racks, taking trays and other containers in a series of interchangeable sizes, has been installed in the extensive reserve storage-space in the basement and in seminar and research rooms, which allows for complete flexibility in the use of a very wide range of archaeological material. The method is economical of space and provides amply for future expansion on a basis which will not require to be changed as the teaching collections grow. Another feature of the building is a series of small research rooms which can be allotted as required to higher degree students and others whose requirements may include facilities for laying out research material in circumstances in which it can be left undisturbed if necessary for period at a time. The library is established on the first floor: here, too, expansion is possible on a scale that was out of the question in Regent's Park.

The ground floor is devoted to the administrative offices, a lecture hall seating about 120, and a vestibule which is intended to serve also as exhibition space. The Institute does not, of course, function as a museum in the ordinary sense, but it is hoped from time to time to stage here exhibitions of a specialized archaeological nature, the results of expeditions and the like, which cannot be provided for elsewhere.

No sensible person would regard the physical advantages conferred by this magnificent new building as in themselves a cause for self-satisfaction. A fine building is not a substitute for teaching well-done or research actively pursued; and the Institute is conscious of targets not yet achieved. In its spaciousness, however, the new building makes a number of developments possible that were ruled out before; in its siting it should lead to closer contacts with the rest of the University and make for the fuller use of the Institute and its facilities.

EXTRA-MURAL ARCHAEOLOGICAL TRAINING CENTRE, BIRMINGHAM

The opening on 7 June of the new Archaeological Training Centre at Wroxeter by Sir Mortimer Wheeler marked a significant stage in the development of the plans of the Department of Extra-Mural Studies of the University of Birmingham. Already over the last eight years there had been a gradual expansion in the teaching of archaeology to adult students at various centres in the West Midlands. Out of the original classes have emerged self-organized groups and local societies capable of carrying out programmes of field-work and excavation in their leisure time under the direction of the staff tutor. Experience has shown that evening lectures and private reading and study in the winter, with these various summer activities are not sufficient in themselves to instil the discipline and experience required for excavations of high standards. Intensive training can be given only under specially arranged conditions such as no normal excavation, usually with a time limit,

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can provide. The Wroxeter project has developed out of the valuable experience of the pioneer school at Great Casterton under the inspired direction of Dr P. Corder and organized by the University of Nottingham. The site of the Romano-British town of Wroxeter offers several advantages. There is plenty of space, there are buildings of various kinds, domestic and public, timber and stone. The complications of structural relationship and the vertical stratification in a depth of 12 to 15 ft. set problems in excavation, recording, drawing and interpretation from which the student can emerge with confidence yet humility. The courses are arranged at three levels, taking the student through the basic elements to a broader understanding of the main principles of archaeological techniques, emphasizing the fundamental importance of preparation of material for publication. Finally, each student is responsible for a piece of work brought to this ultimate stage.

The new Centre and the nearby hostel accommodation at Preston Montford are designed with the courses to help the amateur to equip him to carry out his own local work more expertly and so with a wider appreciation of the problems involved. The Foyle Centre consists of two buildings, one a lecture room which can also be used for drawing and study and the other for washing, marking and sorting pottery and small finds and includes also a darkroom and tool store. The number of students is limited to twenty-four, except for the advanced course of fifteen, and the staff will normally consist of two for direction and teaching and two for technical work. Only in this way can each student receive the requisite individual attention. A comprehensive library of excavation reports and background studies has been gradually amassed.

It remains to be seen what kind of impact this will have over the years but already very useful work is being done on the defences of Roman towns. Perhaps the greatest challenge comes from the rural sites. Aerial reconnaissance sponsored by the Department has led to the discovery of a large number of crop marks along the Severn and Avon valleys. In these areas our knowledge of prehistoric and Romano-British settlement is limited to a few casual finds. Work carefully planned and organized on a series of small-scale excavations would undoubtedly have the effect of revolutionizing our knowledge of the early development of these areas and help to explain similar phenomena in other parts of the country. The amateur can still play an important part in British Archaeology, the opportunities are very great and the enthusiasm already there, and with centres like Wroxeter the training in techniques and discipline can be acquired at a modest outlay.

GRAHAM WEBSTER

GORDON CHILDE MEMORIAL

A number of suggestions have been made that there should be some form of memorial to Professor Childe, and it is thought that many people who knew him would wish to help bring this about. The Committee of Management of the Institute of Archaeology has therefore decided to invite contributions to a memorial fund. Decisions on the exact form that the memorial should take have been left until the support forthcoming for it is more certainly known; at the present time the suggestions are that a memorial lecture might be instituted, or a fund established to provide grants in aid for foreign travel. It is thought that either proposal would have commended itself to Professor Childe, for with a sufficient endowment the Institute would be able amongst other things to maintain and strengthen just those links with foreign scholarship which he always had so much at heart. The final decision in this matter will in due course be communicated to subscribers; in the meantime, donations should be sent as soon as possible to: The Secretary, University of London Institute of Archaeology, 31-34, Gordon Square, London, W.C.1.

Reviews

ZIMBABWE CAVALCADE. By B. G. PAVER. Foreword by Sir Roy Welensky. Cassell, London, 1957. pp. vi + 217, 12 plates, 3 maps. 18s.

This is the revised edition of a book, first published in 1950, based upon the questions aroused by the ancient remains of Rhodesia and the various attempts to explain them. It traces the 'story' of that country from remote antiquity till the time when archaeology began sifting the real from the imaginary. If spurious reports and speculation long ago discredited were eliminated, the book would be half its length, and, from the point of view of the uncritical reader, much less interesting.

The keynote is mystery and romance. In the author's prefacing words 'it is neither anthology (*sic*), archaeological treatise, travel book or tale of fiction'; and it is addressed to those who, faced with what seems a queer alternative, 'prefer anecdote to archaeological dogma'. A review in a serious archaeological journal, whose founder and great editor coined the epithet 'mystery-mongering', may seem, therefore, misplaced. Yet the book's lively narrative style, apt illustrations and over-simplified handling of the problems with which it deals, will have earned it a large and mainly uninformed reading public; however, a small minority who read professional reviews may care to know something of its popular essence.

Its interest for the archaeologist lies chiefly in the light it casts upon the understandable bewilderment, scepticism and vexation felt by the non-archaeologist, exemplified by the author, in face of very remarkable material remains the age and origin of which have been stated, with vocational authority, to differ from what he had hoped. The natural inclination of the uninformed but interested onlooker is to require of archaeology confirmation of pre-conceived opinions. He is delighted when successive professional results do not fully confirm each other, for does this not prove they are unreliable and that the layman's opinion is as good? Being unversed in archaeological methods and criteria, both in their strength and their admitted limitations, he looks for speedy answers to the historical problems in question (designated as mysteries); and not only demands a full explanation of 'when exactly' and 'how' relics of antiquity, in all their infinite complexity, came to be where they are, but also their genesis and causative sequence. That archaeological research, even into literate cultures, can by its very nature only reach its conclusions cumulatively, and that these can seldom, if ever, be absolutely final, tends to be forgotten. The archaeologists' revision of each other's findings is regarded as a sign of weakness and error, whereas the archaeologist himself regards it as a normal process in the always slow advance to more accurate knowledge, common both to the humanities and to science. What the layman regards as 'archaeological dogma' and resents, appears to the archaeologist as the conclusion he believes may be legitimately drawn from his observations: moreover, it is as much his duty to publish his conclusions, however uncongenial, as to pursue the research which made them possible.

These, and other misconceptions colour the background of the book. Upon more factual matters a few comments may be made. Mr Paver declares that those interested in the ruins 'must' favour either an 'ancient or a medieval date', and adds that the

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adherents to the 'ancient theory', who are in a majority, favour dates between 1200–900 B.C. But why 'must'? There are a number of serious students in and out of Rhodesia who favour, on fair evidence, a post-medieval age even later than MacIver's. In any case, the sooner the practice of treating the date of the stone buildings as a unified problem is abandoned the better. They were almost certainly erected over a long period: even Zimbabwe itself is unlikely to represent the building effort of a single period; and, despite a radio-carbon 'date', it would be unwise at the present stage to extend this still solitary result beyond the single wooden beam in the foundations of a major wall of the Elliptical Building. To this C 14 test Mr Paver gives prominence and detail. It is therefore unfortunate that in claiming it as 'unchallengeable' for Zimbabwe as a whole, he should introduce a fixed date of A.D. 591 ± 120 , and confuse it still further in another passage by quoting the 5th century A.D. (The dust-cover further queers the pitch by quoting 500 B.C.) This is therefore neither accurate in itself nor is the matter nearly so simple as assumed. The average of two Chicago C 14 tests, taken in 1952 and 1954, was stated by Libby himself as A.D. 574 ± 107 (*Annual Report*, September, 1954). The London tests, carried out by a more refined method, yielded a date of A.D. *circa* 710 ± 80 (Zeuner, *Radio Broadcast*, 7 February, 1955). The results therefore support each other within the standard error of counting radio-carbon disintegration. It is impossible at the moment to be more exact, and it is too soon for Mr Paver, not unwilling to undermine archaeological conclusions—is not anecdote preferable to archaeological dogma?—to declare that whereas the present reviewer pushed back MacIver's earliest date from the 14th to the 8th century A.D., the C 14 test has now pushed the 8th century suggestion 'back to the 5th'.

Other considerations should also be taken into account. In the first place Prof. Zeuner made it clear that the wood had been contaminated by insects, which made his C 14 count a *minimum*. In the second place, to quote him, 'it has been found that the outer, younger, layers of wood, give later dates than the centre of the tree . . . this causes an archaeological difficulty, for the beam can only have been used a fair number of years after the radio-carbon date provided by the wood. So actually the date of the ruin may be *younger* than the radio-carbon date, though not necessarily by much.' More recently a fresh element of uncertainty has been introduced into the C 14 datings. This is more difficult to appraise and must be held in the suspense account for the time being. It concerns the properties of tambootie wood from which the tested beam was made. A fairly recent book, *Trees of Africa*, by R. A. Galpin, questions whether this wood, *Spirostachys africana*, is ever felled by natives until dead on account of its irritant sap. As the average age of the larger trees is estimated by this authority to be perhaps 500 years or more, the effect on the C 14 dating of the beam by this botanical statement, if confirmed, is great.

In his handling of the dating evidence therefore Mr Paver, as in much else, builds too much and too loosely on too little. In another context, for instance, he gives the circumstantial account of the recent find in an ancient mine-working of a 'unique carving' in soapstone, representing a pair of human heads 'not negroid . . . reminiscent of Mayan sculpture'. From the photographs which have reached this country it seems desirable to treat this object with considerable reserve. If genuine, its affinities cannot fail to be recognized sooner or later.

Karl Peter's famous *ushabti* is again resurrected under Flinders Petrie's aegis. Petrie pronounced it genuine, but he never, despite Peter's assertion, accepted it as emanating from Rhodesia: an archaeologist requires more than the word of a disreputable individual as to provenance.

These, and similar trifles, tot up to a distorted view of Rhodesia's past. It is quite complicated enough without them.

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It is comforting to see however from this book that the days are past when Sumerians, Egyptians, Dravidians, Arabians and Israelites of Solomon's days are specifically invoked as the authors of Rhodesia's ruin-complex. In Mr Paver's words, echoing Frobenius, Dart and Breuil, 'the evidence slight as it is, appears to indicate that the Zimbabwe ruins represent the climax of a succession of earlier cultural contacts between foreigners and local natives' (elsewhere he asserts that the Bantu were not across the Zambesi before A.D. 1000. Surely an open question?) 'This succession of contacts may go back fifteen hundred or five thousand years. It may have been with all or any of the peoples of the past. . . .' Oddly enough a more restrained version of a not dissimilar thought finds expression in another book, now sadly out of date and print, where it is ventured that 'at an epoch in the world's history as late as that to which we date the ruins (i.e. Zimbabwe foundation, *circa* 8th-10th century, A.D.) no-one would deny the possibility—rather would they urge the inevitability—of exotic stimuli, physical and cultural, received and absorbed during ages of coastal contact with alien trading peoples'. (*Zimbabwe Culture*, p. 7.) And in the same work there are passages urging coastal excavations, and others stressing the darkness enveloping the genesis of the Bantu tribes 'that congerie of Hamitic-Negro peoples, gathered together in the scientific net of linguistics, but escaping capture through its wide mesh which seeks to draw them on board for closer racial dissection'.

In two respects, in addition to writing a deliberately controversial popular book, Mr Paver has done a real service to archaeology in pleading for two long-overdue reforms. One is the erection at Zimbabwe of a modern, lively, museum in which the thousands of visitors might study in reproduction that which has been widely dispersed, as well as the remaining fruits of past and present research. Much of what I, the reviewer, left behind at Zimbabwe in 1929 in the then custodian's charge—the boxes of stratified sherds, specimens of M'pani wood hut-poles, bones—which would now be so valuable for comparative and C 14 purposes, has apparently ceased to exist under the haphazards of casual storage.

The second is the plea for a permanent 'top-ranking archaeological and anthropological team engaged in an overall Archaeological Survey', instead of the existing more or less single-handed efforts. Meanwhile we may be thankful that carefully planned and executed field-work of major importance has been, and is being, done with the insufficient budgets of the Commission of Natural and Historical Monuments, and the National Museum of Southern Rhodesia. In this connection the foundation of an equally essential British Institute of Archaeology in East Africa, when it materializes, cannot fail to help the eventual solution of archaeological and ethnic problems in regions such as Rhodesia outside its immediate sphere of operations.

The Bibliography appended to *Zimbabwe Cavalcade* ends in 1949. A great deal of importance has appeared since then.

February 1958

G. CATON THOMPSON

ROMAN SILCHESTER. By GEORGE C. BOON. *Max Parrish*, pp. 245, 21 plates, 38 line drawings and folding map. 25s.

This is the most complete account of a Romano-British town yet produced, and as such has an importance beyond its immediate subject. As Mrs Cotton points out in her introduction, Mr Boon is admirably equipped for the task, since he has not only excavated at Silchester but has spent six years on and about the site and in classifying and cataloguing the vast body of material recovered in the earlier digging.

After a brief introductory section dealing with the story of the town's exploration, the book falls naturally into three parts. The first (Chapters II and III) deals with the history of the place, beginning with the Iron Age and running through Roman times into the

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Dark Ages; the second (Chapters IV to VIII) describes and discusses the known remains and the evidence for the way of life of the town's inhabitants; while the third (Chapter IX) briefly sets the town in its provincial context by noting the courses and destinations of the roads that radiate from it.

Of these sections the first, which relies largely on work still in progress, is inevitably the least definitive and the most open to criticism. The author realizes this, but there are indications that in some cases the realization came after the first draft was made and as a result statement and hypothesis tend to be confused. Much more definite evidence is required before we can accept as Belgic an enclosure with four vertically opposed entrances and with *cardo* and *decumanus* at right angles to one another. The entrances themselves are not, as drawn, 'inturned' in the meaningful Iron Age sense of the term, and since the ditch was filled from the rampart, which in turn contained whatever was in the gravel from which it was constructed, the argument based on the proportion of Iron Age and Roman sherds recovered from the ditch filling is unconvincing. Again, despite the grandeur of its two mile sweep, it is hard to see the 'Outer Earthwork', especially with its Rampier bulge, as an element in Hadrianic 'correction', and the reasons for abandoning Mrs Cotton's earlier, Neronian, date are not altogether clear; the line of the road from Winchester strongly suggests that the earthwork was there before it. The sequence of the earthworks, in fact, is still not settled, and before it can be more attention must be given to the numerous dykes in the area. On the analogies of Camulodunum and Bagendon it is not surprising that they do not fall easily into an intelligible pattern, but it is in them, rather than in any regular *enceinte*, that we may expect to see Belgic Calleva.

On the other hand, as Mr Boon points out, there may have been not one Belgic Calleva, but two—the Atrebat, attested by the appearance of the name on coins of Eppillus, succeeded by the Catuvellaunian, implied by the spread in this area of coins of Cunobelin and Epaticcus. He is inclined to attribute most of the known Belgic material to the latter, but if he can relate the two archaeologically it will be of immense value to our understanding of the period between Caesar and Claudius. In the meantime, the story is hypothetical and the evidence inadequate. It is, for example, a little rash to deduce a pre-Commian Atrebat concentration in the middle Thames area from the distribution of triple-tailed staters (Mack 58 and 59) while disregarding the related quarter-staters (Mack 63-70), which are so plentiful in Sussex that Mack was led to attribute them simply to the 'Regni'.

The central portion of the book is excellent and contains not only a summary of the results of earlier work, related, where possible, to the chronological sequence, but also a number of corrections on points of detail. It is only when we reach Chapter VIII that doubts begin to arise. Silchester is still presented as a 'garden city'; this is a tenable view although, possible timber buildings apart, Mr Boon himself draws attention to the surprisingly large areas left untouched by the earlier excavations, and a fourth century corn-drying oven (p. 179) is hardly evidence for the town's character in its heyday. But he goes further than this and sees it, like a mediaeval village, as wholly dependent on its own, purely local, resources. This view is related to his earlier statement (p. 76) that, if it was not a *municipium*, Silchester was 'merely a *vicus* of the *civitas*'. But the legalistic argument must not be pressed too far. As Calleva Atrebatum and the seat (presumably) of the tribal *ordo*, Silchester had a sort of primacy and for practical purposes its *territorium* was the whole area of the *civitas Atrebatum*. In concrete terms, a decurion with a town house in Silchester might have owned lands anywhere in the canton and whether or not he ultimately converted one of his farms into a country seat he would not have had to rely on a cabbage patch just outside the walls for the maintenance of himself and his household. Nevertheless Mr Boon has done a great service in drawing attention to the need to investi-

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gate the suburban arrangements of our Roman towns, and Silchester, by reason of its modern isolation, is about the best subject for such an enquiry.

The book is nicely produced, very well illustrated and reasonably priced. The index could be improved—the Neronian stamped tile, for example, is mentioned on p. 69 as well as on pp. 108 and 226, and the Imp Stone (p. 204) is not listed in the index at all. More important, it is extremely difficult to refer to the notes, and a great deal of exasperation could be prevented by giving them the same page headings as the chapters to which they refer. These are matters for the next edition, for it is greatly to be hoped that having laid so firm a foundation Mr Boon can spare enough time off from Isca to continue the reconstruction of Calleva.

A. L. F. RIVET

AKU-AKU, THE SECRET OF EASTER ISLAND. By THOR HEYERDAHL. *Allen and Unwin*, 1958. pp. 367, 58 plates, 6 figures. 21s.

The archaeological results of Heyerdahl's latest expedition to Easter and other Polynesian islands are not yet ready for publication, but a foretaste of them is given by this attractive popular book, which serves a useful purpose by providing the background of the work. The plates, which are all in colour, appear in general to be very good, and I trust that the exception which shows Heyerdahl with red hair is not designed to foster the idea that the red-haired strain in the population may be descended from Norwegian Vikings!

Before attempting to outline the results, it will I think be timely to recall what Heyerdahl has achieved so far. A very strong objection which was originally raised to his theory that Peruvian Indians had reached Polynesia in pre-Columbian times was the inadequacy of the balsa raft for long sea voyages. The voyage of the Kon-Tiki in 1947 was the answer to that. At the 30th International Congress of Americanists at Cambridge in 1952, the climate of opinion was still highly critical. As I remarked at the time, in a review of his book, *American Indians in the Pacific*, the problem is primarily an archaeological one, and as such it involves chronology. Heyerdahl realized this and he has gone on to tackle it from that point of view. He began in the Galapagos Islands, where he unexpectedly found Peruvian pottery covering a considerable span in time, which was evidence for repeated visits to the islands in pre-Columbian times. Shortly afterwards, with the help of Sr Emilio Estrada he recovered the lost art of steering a balsa raft by manipulating the centre-boards and the sail, and so showed that these rafts could be handled as easily as any boat and could sail in any direction, a possibility which he had not realized when the Kon-Tiki drifted westwards with the current and nearly came to grief at the end.

Easter Island was clearly the next step, and we hoped that he would try his luck there, not realizing perhaps the difficulties which would need to be overcome. Communication with the island is difficult, and he had to hire a Greenland trawler and fit out a full-scale expedition, which included four archaeologists experienced in the American field. The results, which depend on radio-carbon dates, show a surprising depth of occupation, with three distinct periods. To the oldest belong a few statues of types unknown on the island until the expedition excavated them, associated with walling made of massive squared stones, which share the Inca characteristic of fitting perfectly together although I can not match the type exactly among Classic Inca masonry, and it must be pre-Inca by many centuries if the dates are anywhere near right. It is not yet clear how closely this stage is linked with the carbon dates, but a date of A.D. 800 appears to fall somewhere in it and the island seems to have been occupied by about A.D. 400. It is claimed that the statues resemble examples which are reckoned typologically to belong to an early stage at Tiahuanaco in

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Bolivia, and if this is so, the dates are reasonable. The statues of the two areas certainly have features in common, but an opinion as to whether the resemblance is close enough to point to a direct relationship must await fuller publication.

To the next stage belongs the well-known Easter Island type of statue, and many of these were intended to stand with their backs to the sea on platforms which were sometimes made by pulling the old fine masonry about and adding to it in a crude fashion. These statues were being made in the 15th century, but how much earlier they began is not clear. The end of the period they belong to is connected with a fire in a defensive ditch, of which a tradition survives, which is carbon-dated to the 17th century, not so long before the visit of the first Europeans in 1722.

The last period is that of the present inhabitants, who are believed to have fought and massacred their predecessors, although it is claimed that a proportion survived and intermarried with them. Heyerdahl is quite ready to admit that these last people came from the west in Polynesian canoes. Until well on in the 19th century, perhaps later, the islanders had the habit of making a wide variety of curious small stone carvings, which they regard as belonging to their familiar spirits and those of their ancestors, and hide them in the extraordinarily inaccessible caves which abound on the island. A lot of Heyerdahl's energy was spent in ferreting out these caves, in which the people had sometimes lived in times of danger; no foreigner had ever heard of them before, and he only succeeded in finding them because his previous experience in Polynesia had given him a close sympathy with the people, who, without too much prompting from him, became convinced that he possessed an *aku-aku* or familiar spirit more powerful than theirs, hence the title of the book.

It may be felt that the emphasis which is placed on the carving of stone statues in human form as the strongest piece of evidence for a link between eastern Polynesia and Central and South America is exaggerated, but its weight will be greatly increased if it is proved to get later as it is followed west from island to island, as is suggested by the date of A.D. 1300 which is given for the oldest Marquesan statues. If the resemblances to Tiahuanaco prove acceptable, it may be conclusive. In this connexion, it is the oldest statues in Easter Island which are of interest; Heyerdahl no longer claims a specific resemblance between the later, typically Easter, ones and any known examples elsewhere. Definite evidence of ancient pottery is still lacking on Easter Island; if there were any, it would almost certainly have been imported because suitable clay is practically lacking there, so large quantities could not be expected, and sherds may yet be lurking on sites which were not excavated. A piece of positive evidence for migrations from Peru is the presence of the fresh-water reed, *Scirpus totora*, from which the reed boats of Lake Titicaca are made. They were used for making rafts on the island until comparatively recently. Heyerdahl has found that reed boats are seaworthy, surprisingly durable and much faster than balsa rafts, and there is a hint that he suspects that they were used for voyages from Peru.

The expedition has made important contributions to knowledge. Not only has the archaeological succession of Easter been established, but useful facts have been discovered about that on other islands. Material for a pollen-grain profile has been obtained, and blood samples taken. Interesting experiments were made, which showed how the statues could have been carved with stone tools and raised by a few men. The famous script, which must surely belong to the latest period, did not escape attention, and a manuscript was obtained which may assist in its decipherment. Attention has been drawn to a somewhat neglected problem of Andean archaeology, namely the chronology and distribution of stone statues and masonry. A good many of Easter Island's secrets remain, but some will be clarified by the reports which are eagerly awaited.

GEOFFREY BUSHNELL

REVIEWS

BORNHOLM I FOLKEVANDRINGSTIDEN. By OLE KLINDT-JENSEN. (*Summaries in English.*) *Nationalmuseets Skrifter, St Ber. II; National Museum, Copenhagen, 1957.* pp. 323, 200 + 25 figures, 9 plates (1 coloured). 75 Dan. kr.

In this beautifully produced volume Dr Ole Klindt-Jensen presents us with a well documented picture of life on the island of Bornholm during the Migration Period between around A.D. 400 and 600. Much of the information on which he relies was obtained from his own excavations on the island carried out, with help from a number of colleagues, between 1949-53, but the author has also made an exhaustive study of material in the National Museum at Copenhagen, in the Bornholm Museum at Rønne and in many foreign museums, as well as of the literature. The volume is lavishly illustrated and its value is further enhanced by contributions from specialists, notably those by Hans Helbaek on cultivated plants and by Ulrik Møhl on the animal remains, the latter being especially noteworthy for its study of variations in the size of the more important farmyard animals extending from Mesolithic to modern times. The text is made available to English readers by adequately well-prepared summaries; the captions to illustrations are also translated and there is a useful index.

The picture we are given is that of peasants living on family farmsteads, cultivating cereal crops and keeping livestock, predominantly cattle and sheep but including a fair number of horses and swine; hunting was of little importance apart from seals, which were doubtless taken primarily to provide oil for lamps.

Apart from bog iron, the supplies of which were hardly adequate for their needs, the islanders had to depend on trade for such necessities as metals and salt, as well as for luxuries and trinkets of all kinds. The archaeological material from the island shows clearly enough that the Bornholm farmers were in contact with the Baltic coasts and, by way of the Don and Vistula, with the Danubian area, meeting place of German, Roman and nomad. Moreover, there are abundant signs that not all these contacts were peaceful: the indications are that the island, like Öland and Gotland, was attacked by sea-raiders; and coins in the gold hoards suggest that the crisis occurred in the reign of the Emperor Anastasius (A.D. 491-518).

As an example of the author's insight and range, one may quote his treatment of the topic of horse-sacrifice. This was suggested to him by the discovery in a pit just outside a building of the Migration Period at Sorte Muld of the remains of a horse's head surrounded by severed feet and a pelvis, none of the bones having been gnawed or split for marrow. In interpreting this enigmatic find, which nevertheless finds parallels in Denmark, Klindt-Jensen ranges over the archaeology of the whole of Central and Eastern Europe and of Western Asia, and interprets this in terms both of early travellers' accounts and of ethnographic literature. From this he not merely infers the practice of horse-sacrifice during the Migration Period in Denmark, but connects it with other evidence for the irruption into the Western World of nomads from the east. In such a fashion can a few horse-bones be made to illuminate the past.

One may conclude by stressing that this is a book that could be read with profit by anyone concerned with antiquity, since it shows what can be achieved by way of synthesis when all the relevant data have been taken into account. In too many parts of the world, not excluding Britain and many European countries, prehistorians are necessarily still engrossed in the often dreary task of creating the basic framework of their subject, devising chronological periods and distinguishing cultural entities by means of painstaking analysis of forms. It is a rare treat, and indeed a wholesome exercise, to turn now and again to a region such as Scandinavia, where these tiresome preliminaries have to a large extent been dis-

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posed of and prehistoric archaeology has already begun to give place to prehistory. The task of reconstructing a single phase of prehistory is, of course, much simpler than discovering the mainsprings of change, but it is one which nevertheless needs to be undertaken whenever and wherever the necessary information has been made available by the archaeologists and their scientific colleagues. Conversely, it is by visualizing the pre-requisites for such volumes as this that prehistorians in more backward regions can most easily visualize what most needs to be done in their respective territories.

GRAHAME CLARK

ART TREASURES OF THE BRITISH MUSEUM. By GEOFFREY GRIGSON. *Thames and Hudson*, pp. 71, 153 *monochrome* and 6 *colour plates*. £3 3s.

This is a highly personal selection of works of art from the vast collections at Bloomsbury—and all the better for that. As with poetry, so with the visual arts, the more clearly the anthology shows the colour of the selector's mind the more enjoyable or significant it will be for the consumer. The basis of Mr Grigson's choice is energy and intensity of feeling, or as he himself expresses it: 'Many objects of that formal electricity, that energy, feeling, conviction, absorption—nowhere to be experienced more strongly in the Museum than in the Ethnographical Gallery—have been chosen for illustration; and have been preferred to merely rich and rare, merely treasurable art treasure, hoard objects, objects of men's costly ostentation, or objects—of which the Museum has plenty, in common with all the great museums—into which an art tradition declines when, as motives, refinement, sentiment, nostalgia, and mental fulfilment of wishes supervene, and the artists subside into realism or unreality.' As a result, a great many of the objects chosen (and brilliantly photographed by Edwin Smith) would never for a moment have been considered for inclusion a century ago. Yet most are delightful and well worth having drawn to one's attention. Because of the pursuit of energy, there are perhaps rather too many portrayals of animals both naturalistic and grotesque (just half the plates) and rather too many of the sort of little figurines that have the liveliness of child art but not much more (plates 10, 11, 56, 57). Again, it is possible to complain that in what seems to be a reaction from the dominant trend, the pick of classical works is inclined towards the soft and pretty. As for omissions—which every critic has to be allowed to grumble about—although I warmly agree with Mr Grigson's decision that there should be no nationalistic bias—is it not rather perverse to have only one late and untypical piece of Celtic art and nothing Anglo-Saxon at all?

Still, as an anthology, I strongly recommend *Art Treasures of the British Museum*. The notes accompanying each picture are well done, too, and the text, though the definitive sentence quoted above would suggest otherwise, shows far less than usual of Mr Grigson's conscientious affectation of style and peculiarity of syntax. No, it is not the book that makes me feel uneasy, but what the book says about the Museum.

The author is strongly in favour of the Museum remaining, as it was in the 18th century, 'chiefly designed for the use of learned and studious men . . . for their researches into the several parts of knowledge'. Sir Thomas Kendrick, too, in his short introduction supports this view when he says that in the opinion of himself and most of his staff, display to the public should rank only fourth or fifth among their duties.

Quite apart from being provoked by an undertone of complacency audible in both voices, I feel this judgment must be opposed on two counts. The first is that while I, too, am wholeheartedly against a great museum being turned into an educational institution filled with an excess of instructive labels, flags, pointers and hordes of children, this is surely a time when an appreciation of art and cultural history should be spread as widely as possible

if it is to survive at all. It should, then, be one of the *first* duties of a museum to do everything within its means to provide for the enlightenment and aesthetic pleasure of the ordinary public. The second reason for opposing the judgment is the assumption that if the needs of the public are neglected, those of the 'learned and studious' will be better served. I believe this to be the opposite of the truth. In the Scandinavian museums, for example, where the splendid displays put the British Museum to shame, the provision for students, for research, for field work and the happiness of the staff are also greatly superior. These museums are boldly claiming an important place in the life of a civilized community—and are therefore granted sufficient means to maintain it in all aspects of their work. If the British Museum withdraws into a shell of 18th century tradition, a 20th century State is given some excuse for keeping its finances at their present disgraceful level.

JACQUETTA HAWKES

PREHISTORIC MAN IN DENMARK. By KURT BRÖSTE, with J. B. JØRGENSEN, C. J. BECKER and J. BRØNDSTED. 2 vols.; Vol. I text and tables, pp. 159; Vol. II, drawings and plates, pp. 439; Munksgaard, Copenhagen, 1956. Quarto. 230 Danish Crowns. (Vol. I has 46 figures; Vol. II, 265 plates and 115 + vii tables. Vol. III will be on pathology.)

Every time we self-indulgent physical anthropologists pause in our work to enjoy one of the finer things of life, namely a bottle of Carlsberg Danish beer, let us mutter a word of thanks and benediction to the benevolence of those who brewed it, for having generously financed the publication of these two magnificent volumes of a projected three volume work. Although priced at 230 Danish crowns (about £20 or \$60) it must have cost a small fortune to print. Everything about it is large and generous—its type page, its margins, the fount of its type, and the size of its illustrations. Built to recline horizontally on a shelf, it is one of the handsomest publications of our typographically decadent times.

Here we have the definitive publication of every single adult human skull and long bone found in Denmark and still retained there which can be positively identified with the Mesolithic, Neolithic, and Bronze Age populations of that country, exclusive of South Schleswig (Slesvig) and Scania. Four are Mesolithic, four Early Neolithic, and five Bronze Age (all Early Bronze). The Middle and Late Neolithic are represented by 252 individuals; 47 from the Middle, 72 from the Late, and 133 which cannot be assigned to one or the other with certainty, although definitely belonging to the total span of both.

Each skull and long bone was measured by the standard Martin technique. All raw measurements and indices are individually given. Photographs of all specimens are reproduced in Volume II. In the three Neolithic series, large enough for statistical treatment, the conventional biometric constants were calculated; correlations between sets of characters were made, and t-test values determined in comparisons between chronological series. Comparative data from Sweden, Norway, and Germany were introduced where pertinent.

The Mesolithic Danes were found to be heavy boned, large brained, and large faced, while the Neolithic people were mostly of a different phenotype, related to the Danubian folk of Germany and points east, and the Bronze Age specimens show no particular deviation from the Neolithic pattern. However, the Single Grave People of Jutland are not represented at all in the Middle Neolithic, while in the Late Neolithic, although they are present in the series, the authors considered the contents of tumuli in which single graves had been dug on top of dolmen or passage graves to have been so scrambled that they were unwilling to identify a separate series of single grave skeletons.

We know that in Germany the single grave skeletons were different from the other

Neolithic remains in several respects. In Denmark we cannot tell whether they were or not. However, the Late Neolithic series, which includes single grave skeletons, is significantly taller and larger faced than the Middle Neolithic series which includes none of them. Either these changes suggest either that a new element has been added, or that growth changes had taken place in the Neolithic population in response to some dietary or other change; Western Europeans and Americans are going through just such a growth phase today.

It is interesting to note that the famous brachycranial and very rugged skeletons of Borreby were not Mesolithic, as formerly believed, but Middle or even Late Neolithic after all. Their new archaeological booking merely shows that the population of Denmark need not have been geographically homogenous in antiquity, any more than it is today.

For the service of those who can neither obtain a free review copy, spend £20, or see this handsome pair of volumes in a library, it may be stated that the four authenticated Mesolithic skeletons are Stasevang, Kelderød, and Døjringe I and II. The four Early Neolithic ones are Koelbjerg, Ravnstrup, Vedbaek, and Korsør Nor. The five Bronze Age ones are Borum Eshøj A, B, and C, and Nykøl and Viksø.

As usual, the Danes have delivered the goods. This time they have given the world a handsome, conservative, accurate, and invaluable work that everyone must consult who wishes to deal with the materials of the somatology of prehistoric Denmark, Scandinavia, Europe, and indeed the world.

CARLETON S. COON

BRONZE AGE CULTURES IN FRANCE: The Later Phases from the Thirteenth to the Seventh Century B.C. By N. K. SANDARS. *Cambridge University Press*, 1957. pp. xvii + 412, 97 text figures, 12 plates; maps and tables. £5 10s.

It was nearly fifty years ago that Déchelette published the second volume of his *Manuel*, dealing with the Bronze Age. Since that time little has been attempted in the way of synthesis, and what has been done has almost always been the work of scholars from outside France—Savory's studies of the Atlantic west, and Kimmig's on Urnfields in the east spring to mind. Now Miss Sandars has produced a remarkable book which, despite the limitations in its sub-title, surveys the Early and Middle Bronze Age of France before it turns to review in greater detail the material of the Late Bronze Age Urnfield Cultures. Such a survey is of the greatest value, and has not been attempted before.

Miss Sandars begins by considering how the French material can be correlated with the two main European systems for classifying the Bronze Age, that of Montelius in the north and that of Reinecke for south-western Germany (and by extrapolation, for wider areas of Central Europe). Possibilities of correlation lie mainly, of course, in eastern and south-eastern France, with the Rhône Culture parallel with Reinecke A2 at least, if not in part with A1. This is her Period II (Period I being the mixture of Late Neolithic and early imported metal as in Central Europe or in Britain). At the end of Period II she thinks the Armorican Single-Grave culture may begin, but it lies mainly in Period III, a short phase perhaps spanning no more than the second half of the 16th century B.C., parallel with Reinecke A2/B1 overlap, Montelius Ia and B, and the British Wessex Culture. In this connection the author underlines the likelihood of a common Central European origin for the Breton and the British cultures in question, with subsequent coming and going across the Channel.

We then move into the developed cultures of 'Middle Bronze Age' type, well represented at Hagenau where the burials represent an extension of the Tumulus Bronze Age in Reinecke's B1 and B2 phases: Miss Sanders has a valuable discussion of the

complex-bored amber spacers from Hagenau in their wider chronological context. In the west, the Tréboul hoard with its big rapiers and other objects including belt-clasps and a piece of a wheel-pendant or wheel-head pin would belong here, in Period IV. (A set of outline drawings of the objects in this hoard have now been issued by the Faculty of Science in the University of Rennes in their duplicated *Travaux du Laboratoire d'Anthropologie et des Musées Préhistoriques* for 1956.) When we move into Period V, a transitional phase equivalent to Reinecke C and D, we may note in passing another recent publication of a site referred to by Miss Sandars, the cemetery of La Colombière, Champlay, Yonne, where pre-war finds included a most remarkable burial with a pair of fine bronze anklets of Hagenau Type 3 and an extraordinary 'diadem' made of a boar's tusk mounted in an elaborate spirally-ornamented setting of bronze wire. (B. Lacroix, *La Nécropole Proto-historique de la Colombine*; Paris, 1957.)

In the first Chronological Table in the book, the divisions of the French Bronze Age just mentioned are given, with their correlates in other regions, and Period V brings us to the Late Bronze Age, and the First Urnfields characterized by 'Poppy-pins'. But when we come to the main thesis of the book, the Urnfield Cultures in France, we find in Chronological Table II (p. 118), that the numbered periods have been abandoned, and we have a sequential classification using a version of Kimmig's division into First, Second and Third Urnfields, running (in the Reinecke scheme) into Hallstatt B2, and this is expanded into a more detailed picture for France in Table III (p. 339), where the French sites are set against the Reinecke scheme, and can be given actual dates from the mid 8th century B.C. onwards. The complexity of the Late Bronze Age material obviously demands a somewhat different treatment from that of the earlier phases, but it is a pity that a consistent system of nomenclature could not have been devised to cover the whole sequence.

The main part of the book is occupied with a discussion of the French Late Bronze Age in terms of the successive phases of Urnfield influence and colonization. It must be remembered that Miss Sandars has deliberately limited her survey to material of Urnfield origin or derivation: with her book we can now take Miss Margaret Smith's general survey of the Central European Urnfields and their western connections in *Zephyrus* VIII (1957), 195-240. The problems involved in the western French Late Bronze Age and of the great hoards have been 'deliberately neglected' by Miss Sandars (p. 192), so that we must wait for a general study which will continue the work of Savory (*Proc. Prehist. Soc.* XIV (1948), 155; cf. *ibid.*, XV (1949), 128) and follow up Hencken's comments on carp's-tongue swords and the fibulae in the Nôtre-Dame-d'Or and Vénat hoards. (*Zephyrus* VII (1956), 125-178.)

Indeed, when we look at Miss Sandars's maps in the Late Bronze Age (maps VII-XIII) we have to remember that on them we are shown a selective distribution to illustrate a point, not an overall picture of types coeval across the whole of France. In the Early and Middle Bronze Age maps one is struck by the concentrations in two areas—an extension of the Central European province in Alsace, Burgundy and the Rhône Valley on the one hand, and Brittany on the other. With what do we fill the blanks on the maps? The answer is, of course, not a difficult one to arrive at. These are the areas of survival of 'Neolithic' and 'Chalcolithic' cultures, including the makers of more than one type of chambered tomb, which did not participate in the metal trade with the Rhineland or with Britain which gave rise to Rhône or Armorican culture-centres. Such a concept of diverse levels of technological development existing side-by-side within an area as large as France is not a difficult one for British archaeologists to accept, accustomed as they are to such diversity within the much smaller area of their own country. For others, with a liking for a neat chest-of-drawers classification applicable to a whole country, or with minds still spell-

bound by the words *néolithique, chalcolithique, Age du Bronze*, it may be the more difficult to grasp. But the recognition of such a state of affairs in many periods of prehistory seems the only hope of constructing a reasonable picture of the communities involved.

It is impossible to end this review without not only congratulating Miss Sandars on a most remarkable piece of work, but touching once again on the circumstances which have led to yet another piece of major synthesis in French prehistory being undertaken by a scholar from outside the country. Our French colleagues may well feel uneasy about the situation, and on our side we can only say in all fairness that we share the feeling. We may expect Miss Sandars's book to prompt critical reviews in France (though some of the preliminary sniping that has already taken place is not really of a nature likely to advance scholarship), and if such criticism is well-informed, and based on as wide a knowledge of the European Bronze Age as the author's, valuable new aspects may emerge. At least one hopes that it will stimulate work in French prehistory on comparable lines, and at a comparable standard of excellence.

STUART PIGGOTT

ANCIENT LANDSCAPES: STUDIES IN FIELD ARCHAEOLOGY. By JOHN BRADFORD, M.A., F.S.A. *G. Bell and Son Ltd., London, 1957. pp. 297, 25 text figures and 75 plates. 84s.*

During the late war many British archaeologists were engaged in the interpretation of air photographs in many parts of the world, and they made a solid contribution to victory. This was no accident. The use of air photography in reconnaissance was first practised during the war of 1914-18, and after that struggle, largely through the efforts of the late Dr O. G. S. Crawford, its most dramatic application to peace time pursuits was in furtherance of field archaeology. By 1939 this new technique had been fully proven in Britain, though its use still lagged behind in other countries. Most British archaeologists were familiar with it and so found a new field for their skill in a war in which aerial reconnaissance played a dominant part. Mr Bradford was one of these, and it was his good fortune to be employed by the Army in a theatre, the Mediterranean, in which the potentialities of archaeological reconnaissance from the air are enormous. He did not neglect his opportunities and hence this book.

During the past twenty years archaeologists have been realizing that the old approach to archaeological problems by random sampling is not enough. We require a broader view of ancient societies than can be got by the excavation of burials and the testing of sites for date. We also require our view more quickly if the increased exploitation of land is not to blot out for ever what remains of ancient ways of life before they can be comprehended. Field archaeology on the ground can do much and skilful and selective total excavation even more, but it is the wide-ranging aerial view which, under favourable conditions and correctly interpreted, can pull the whole thing together. In fact, as Mr Bradford urges, it is the piecing together of social units and the reconstruction of ancient landscapes which are required to make the past live again.

His Army duties led him to examine the air-photographic cover taken of Italy during the past war and provided him with his point of departure, for not only was extensive cover available in Apulia showing the use of that land from Neolithic times, through the Romans, to the time of the Hohenstaufens, but in Tuscany the great cemeteries of the Etruscan cities could be plotted fully for the first time, and in the valley of the Po the undying mark of Rome appeared in vast areas of surviving centuriation.

It is proper to say here that this book could not have been written without the co-operation of the Air Ministry which made its vast mass of air cover available for study.

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This is in strong contrast to the attitude of its predecessors after the first war who were very resistant to the use, or even the preservation, of much which was taken then, particularly in the Middle East. Thus far have we progressed.

The book naturally falls into two parts, a statement of principles and methods followed by a number of examples. The first is an able discussion of air archaeology, its purpose and practice, with full attention to all technical questions. Nothing equal to this has been written to date, and it is surprising that we have had to wait thirty years since Crawford and Keiller's *Wessex from the Air* fairly launched the new method.

Bradford's treatment of his subject is comprehensive for the Mediterranean area and Western Europe; most of the possible conditions which can exist in any formerly inhabited parts of the world are covered in one way and another. Whereas till now most discussions of the technical possibilities of air photography as applied to archaeology have been concerned with a limited area of Western Europe, Bradford now brings in the results of his experience in Greece, Italy, and North Africa along with examples from Turkey, Iraq, Iran, North America, and even further afield. Of particular value is the section on the scope and limitations of the method and also on the need for terrain study to bring out the full possibilities of an air-photograph, i.e. not only to show where the main centres of archaeological interest will lie, but also where *not* to look for them. Hence a huge saving of time and effort in what may be very difficult country. The total result is almost indigestibly rich, and even in this sector of the book the reader is left with an almost boding sense of what the skilful application of this technique must bring forth in the future.

Mr Bradford then proceeds to discuss the methods and problems involved in reconstructing a prehistoric landscape, using the Apulian air cover as a text. Here, as in other Mediterranean areas, he is fortunate because a happy combination of soil and climatic factors bring about a remarkably complete demonstration of the former use of the land to anyone who has eyes to see, but this cannot be the case everywhere as he is careful to point out. He goes carefully into both the strategy and tactics of air reconnaissance and defines the rôle of excavation which alone can be the final arbiter in developing detailed knowledge of the prehistoric communities whose general relation to their habitat has been shown by the air photograph.

The next chapter deals with the special problems of Etruria. Here the recognition of the location and detailed arrangement of the great cemeteries attached to the ancient cities of the land is his principal achievement. Plate 31 demonstrating the existence of dozens of levelled Etruscan tumuli with their dimensions and entrances all fully apparent under a featureless grassy field may be taken as summarizing the essential features of this work which has been followed up vigorously in the field, not least by the new technique of periscope photography.

Chapter 4 deals with the recovery of the Roman rural landscape as revealed by centuriation and other systems of 'Limitatio'. The extensive survivals in Italy and North Africa come as no surprise, for much was known about them before, but the air photograph scores a signal triumph as a means of revealing them in all their dramatic extent and also as an aid to their relationships with the historical record and with the communities which they served. The novelty in this chapter is the study of the centuriation which pushes inland over most difficult country from Roman Iader (Zara), Salona, and elsewhere along the coasts of Istria and Dalmatia. Metropolitan France is, and remains, a lacuna in the widening appreciation of the archaeological uses of air photography and therefore it is good to see that Mr Bradford has succeeded in recognizing what must be a centuriation east of Valence, and that he also knows of significant traces round Orange, Narbonne, and Béziers for future study.

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The work concludes with a chapter on classical and medieval town plans as elucidated by aerial photography. It ranges over a considerable field covering such things as the discovery of the street plan of ancient Rhodes, the city plan of Paestum, Roman town planning in North Italy, further light on topographical problems at Carthage and Ostia, and medieval and early modern fortresses in Venetia. This is full of valuable matter and the demonstration of the ancient topography of Ostia with the ancient coastline, its development since classical times, and the relationship of the various harbours there is a model example of the air photograph's power to bring order out of relative chaos. No future studies of urban development can afford to ignore the air photograph.

It remains to say that the book is excellently got up and contains a profusion of text-figures and plates of air photographs. The latter have been very clearly reproduced and the purchaser may be assured that he will get good value for the stiff price.

C. W. PHILLIPS

ETHIOPIAN ITINERARIES *ca.* 1400-1524. *Edited by* O. G. S. CRAWFORD. *Published for the Hakluyt Society by the Cambridge University Press.* pp. 210. 30s.

Few archaeologists have been so creative as Dr O. G. S. Crawford. It is not only that he made ANTIQUITY. He was primarily responsible for the development of the archaeological use of air-photography. He was also one of the founders of the scientific study of African history and archaeology. He had begun his field work in the Sudan in 1913 and he had worked for many years on the medieval Christian churches and castles there. In 1951 he had published his masterly book, *The Fung Kingdom of Sennar*. It is only fitting that he should be partly commemorated by the posthumous publication of his *Ethiopian Itineraries*.

This is a volume of early Western texts on the geography of Ethiopia, carefully edited and translated and elaborately annotated and discussed. The first chapter deals primarily with two maps, the 'Egyptus Novelo' completed by a Florentine before March 1456 and that of Fra Mauro completed at Venice on 26 August, 1460. Both are reproduced and replotted. Dr Crawford notes how much more detailed was the knowledge at Venice than at Florence. This was perhaps due to the presence in Venice of Muslim traders from Egypt. It would seem that some of these had been concerned in the East African trade. It is curious to note that the existence of the East African trading cities was already known in Venice over forty years before Vasco da Gama rounded the Cape; Kilwa and Sofala are both marked on Fra Mauro's map.

The second chapter is concerned with an extract from the *Iter de Venetiis ad Indiam*, which can be dated from internal evidence between 1382 and 1411 and which is prospectively confidence-inspiring, until it reaches Axum. The next section is devoted to an account by a certain Francesco Suriano of a journey he claimed to have made to the court of Prester John apparently in 1482; it seems probable that he reached the town that he calls *Menna* close to Suakim and gathered rumours and gossip there. The rest of the volume deals with the Ethiopian itineraries compiled at Venice by Alessandro Zorzi between 1470 and 1538.

It is possible that Dr Crawford takes these itineraries more seriously than they deserve. Zorzi derived his facts from Ethiopian monks passing through Venice. Clearly there were language difficulties; he notes of his interview with 'Brother Thomas' on the 4 May, 1524, 'I questioned him as well as I could seeing the little Latin he had'. Clearly his informants were anxious to impress their host with the vast extent of their country and the number of its cities; this seems particularly true of 'Fra Antonio', 'very black with a long beard and long hair and a long and beautiful face'. Axum is described as greater and more

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wonderful than Rome and Ethiopia possesses a University with many faculties. I do not think that Dr Crawford ever shared this reviewer's scepticism. He worked with an unflagging and very loveable enthusiasm on the reconstruction of the itineraries of these travellers in terms of modern Ethiopian geography. He found particular pleasure in his identification of 'Barara' or 'Barar', the mysterious capital of Prester John to which this group of travellers so often refer. This he placed on Mount Borora thirty miles south-east of Adis Ababa. I doubt if this can be sustained; the modern name Borora is irrelevant for it is almost certainly Galla and therefore 17th or 18th century in origin, during an admittedly inadequate archaeological survey I could find no traces of such a site, no other documentary evidence suggests that there was then a static capital. It seems more likely that Zorzi's informants were referring to some frontier camp pitched by the Emperor on the road to Harar during his wars with the Sultanate of Adal. This might explain why all the Zorzi documents are so insistent on the great volume of far eastern trade that came up to 'Barara' from the coast. The volume ends with a very valuable 'Gazetteer' to Fra Mauro's map and with an appendix on an Ethiopian embassy to the West in 1306 and it has been admirably produced. There is only one consistent flaw; Dr Crawford never normalized his spelling of proper names. Three variants are used in the rendering from Amharic, occasionally in the same paragraph. Even the Arabic is idiosyncratic, it is hard to recognize Saif ad-Din Jaqmaq as 'Gakmak'. But the great can be allowed idiosyncracies and no one who remembers O. G. S. Crawford will doubt his quality of greatness.

GERVASE MATHEW

DOLMENS ET MENHIRS. By FERNAND NIEL. *Presses Universitaires de France. Paris*, 1958. 300 francs. pp. 117 (23 text figures).

MENHIRS ET DOLMENS. By P-R. GIOT. *Chateaulin (Finistère), Éditions d'Art Jos le Doaré*, 1957. 300 francs. pp. 37 (17 plates).

The very useful and inexpensive *Que sais-je ?* series published by the Presses Universitaires de France, which already contains many little books of value to students of antiquity, was bound, sooner or later, to have a volume on megalithic monuments. It is a pity that the editors have chosen to write it an author who, to the best of the reviewer's knowledge, has hitherto written little if anything about megaliths and who, from his bibliography and incidental references, is not really very well acquainted with dolmens and menhirs in fact or in literature. His approach is a 19th century one that would have been approved by James Fergusson, and his book, like Fergusson's *Rude Stone Monuments*, suffers from treating of all megalithic monuments at all time from all places. The ogham stones of Ireland, the stone circles of Tibet, the 2,200 dolmens of the Deccan, and the 3,000 strong dolmen cemetery of Roknia in north Africa have, surely, nothing in common except their use of largish stones and their inclusion in this unfortunate book. Monsieur Niel seems to know nothing of the refinements of dating the megalithic tombs of Europe, nothing of the recent work at Stonehenge, and is (perhaps happily) unaware of the complications of megalithic taxonomy and typology. He does give us, however, a useful distribution map of megalithic tombs in France and reminds us that Diodorus Siculus when quoting Hecataeus, might have been referring to Stonehenge, and Skymnos of Chios just possibly knew, in the 1st century B.C., of the *grand menhir brisé* of Locmariaquer as an erect monument—it certainly was broken when Robien drew it in 1727.

It is a pleasure to turn to *Menhirs and Dolmens*, a small book published and illustrated by Jos Le Doaré in that series *Images de Bretagne* which contain so many of his good photographs, and written by Dr Giot with the collaboration of his assistants at Rennes,

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J. L'Helgouach and J. Briard. This is an admirable introduction to the megaliths of Brittany. Giot describes the two main types of Breton megalithic tomb; the passage grave (or *dolmen à couloir*) and the gallery grave (or *allée couverte*). He very properly classifies the monuments usually referred to as *allées couvertes coudées* as forms of passage graves. Quoting the segmented faience bead from Parc Guren in the Morbihan and the amber spacer beads from Lesconil (Plobannalec) in Finistère, he argues that the Breton megalithic tombs went on in use until between 1400 and 1200 B.C., but began about 2100 to 2000 B.C. He distinguishes monuments like Essé and Cournon in eastern Brittany from the ordinary *allées couvertes* of Finistère and Côtes du Nord, seeing their affinities with the great Loire megalithic tombs like Saumur and Mettray. Indeed it is likely that there are many different kinds of monument and several varied ancestries included in the general term *allée couverte* in north-western France.

GLYN DANIEL

THE POLITICAL GEOGRAPHY OF THE YUCATAN MAYA. By RALPH L. ROYS.
Carnegie Institution of Washington, Pub. 613. Washington, D.C., 1957. pp. iv + 187, 17 maps. Paper, \$1.60.

Ralph Roys has brought to the writing of this book an unrivalled knowledge of colonial written Maya and an enviable mastery of early Spanish administrative documents. He has also gone time and again into the less accessible parts of Yucatan to locate, for boundary lines, the sites of numerous 16th century towns which survive only in the retentive memory of the present-day Maya. This happy and rare combination of investigation at desk chair and on mule saddle gives the book its authority and charm.

Roys' chief aims have been to document the boundaries of the sixteen 'provinces' or *cacicazgos* into which the northern half of the Peninsula of Yucatan was divided immediately before the Spanish conquest, to locate the sites of old towns abandoned when the Maya were resettled by the Franciscans for easier indoctrination, and to obtain from the tax list of 1549, made before disease swept off so many, an estimate of the population of the region at the coming of the Spaniards. As corollaries, he uses the tax lists for information on local industries, rights of fishing, etc., and investigates the significance of name groups—the so-called Maya lineages—and their distribution. Clearly, Roys goes much farther than the title of his book would imply; much of his material is for the sociologist or for the historian rather than for the political geographer. There are many leads for future investigation.

The maps of the sixteen 'provinces' are a very important feature. Rough attempts to delimitate the *cacicazgos* appear in the Espinosa map of Yucatan (c. 1910) and in the Gates' edition of *Landa*, published in 1931, but both are unreliable. In contrast, very careful research is at the back of Roys' maps, which, one may reasonably suppose, will stand for all time. From the figures on heads of households in the tax lists of 1649, Roys reaches a figure of just over 250,000 Indians for Yucatan exclusive of the two south-western provinces of Uaymil and Chetumal, but by even that early date, the population had been seriously reduced by smallpox, introduced nearly thirty years earlier, and by flight into unoccupied forest lands to the south beyond the Spaniards' reach.

A most important contribution.

J. ERIC S. THOMPSON

Correspondence

The Editor of ANTIQUITY

SIR.—To date, the only two technical responses to my work on Linear A, are those of Prof. Maurice Pope. The first is his paper read before the 'Minoan Linear B Seminar' at the University of London on 30 October, 1957, of which I have the mimeographed abstract. The second is his criticism of my work, in *ANTIQUITY* of June 1958, pp. 97–99.

His two contributions, like mine, contain a mixture of sound fact and abysmal error. Errors cannot be completely avoided in any pioneer task. Happily, I have grounds for believing that his sins and mine are not mortal but venial.

The first of my articles in *ANTIQUITY* proposed a Semitic *identification* of the Linear A language. (Never have I claimed a *decipherment* of all the texts.) A few assorted words appeared to be Semitic. To cite only one: *ku-pa₃-na-tu*, which Pope on 30 October, 1957, confirmed as Semitic on independent grounds. More important was the fact that three out of five legible pot-names on text HT 31 looked Semitic. If the readings are correct they cannot be loans, because the highly civilized Minoans, with a long ceramic tradition, would not be borrowing *most* of their ceramic terminology from abroad. On 30 October, 1957, Pope added four more probable pot-names to the Semitic list, corroborating, willy-nilly, my position.

A few weeks before my first *ANTIQUITY* article appeared in October 1957 I noted that the conjunction *ū* 'and' appeared in two tablets. At the early date of the Linear A period the only Semitic language with *ū* instead of *wa* for the conjunction 'and' is Akkadian. This tied in with my reading of *ša* 'of, which' (*ANTIQUITY*, October 1957, p. 125) that is exceedingly common in Akkadian, but very rare outside it.

It is unnecessary to bring other sound Akkadian readings into the argument at this point. Ask any Semitist, What is the only Semitic language with *ū* for 'and', and with *ša* for 'of, which'? and he will reply without a moment's hesitation, Akkadian! That is the question I asked myself in August 1957, and I came up with the only conceivable answer. I thought it was important enough to announce in the press, so that everyone might know without delay the Akkadian framework within which the decipherment might at last progress more rapidly. I was not disappointed. Within hardly more than two months, Pope delivered his paper of 30 October, 1957, confirming the specifically Akkadian character of Linear A with new readings of his own, as well as repeating some of mine.

The negative emphasis of his printed critique in *ANTIQUITY* is out of keeping with the constructive, unpublished abstract of 30 October, 1957. Scholarship would have been better served if his three pages in *ANTIQUITY* had been devoted to the valuable contributions he presented orally on 30 October, 1957.

That my September (1957) article was antiquated before it appeared bothered me. But that was the price of progress. I felt absolutely no obligation in my December article, to adhere to all my readings in the September one. I regret only that in changing *ku* to *mu*, and *ki* to *ku*, I committed the sin of retrogression. Months before Pope's criticism came to my attention, I had seen the error of my ways, and had returned to the right path. From the start I was less than satisfied with my readings of *kitmuru*, *mullu* and *kullu*, because I had arrived at them with too much brilliance. In such matters simplicity is better than complexity. I distrust human ingenuity—and most of all my own.

Inherent in all my work on Linear A is the principle of taking each item, or group of related items, on its own merits. I have shunned the setting up of a single cryptanalytic system that stands or falls like a house of cards, because the limited amount of the Linear

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A tablets (with their combined syllabic and logographic script) does not lend itself to methods applicable to a large corpus of purely alphabetic or syllabic material.

There is common to both A and B a means of checking the linguistic identification. Greek words in B pointed to the Greek language, which has been confirmed by a Pylos tablet with Greek pot-names controllable through pot-pictographs. Now the Akkadian words in A point to the Akkadian language, which is confirmed by Akkadian pot-names (for they are Akkadian rather than Semitic in any other sense!) inscribed over pot-pictographs in text HT 31.

In his last sentence Pope challenges me to produce a credible translation of any one tablet. Since early childhood I have felt no compulsion to take dares. At the moment I cannot translate any tablet *completely*. But I can come very close, as I shall demonstrate in an article to be published in the *Journal of Near Eastern Studies* for October 1958. This article will cover not only the cryptanalytic, epigraphic and linguistic aspects of the problem, but also the cultural and historical sides.

Sincerely yours,

Graduate School of Arts and Sciences,
Brandeis University, Waltham,
Massachusetts, U.S.A.

CYRUS H. GORDON.

11 May, 1958.

Book Chronicle—continued from page 186

MYTH AND RITUAL IN THE ANCIENT NEAR EAST. By E. O. JAMES. pp. 352, 1 map, 1 chronological chart. *Thames and Hudson*, 1958. 35s.

ENGLISH TRAVELLERS IN THE NEAR EAST. By ROBIN FEDDEN. pp. 44, and 1 plate (Eric Kennington's pastel of C. M. Doughty). *Longmans, Green and Co.*, 1958. 2s. This is no. 97 in the *Writers and their Work* series published for the British Council and the National Book League as Bibliographical Supplements to *British Book News*. A fascinating account of travel from Henry Muundorn to Freya Stark. It is the first issue in this series to present an appreciation of a *group*, as opposed to a single writer. May we please soon have English travellers in India and China.

AKRAI. By LUIGI BERNABÒ BREA. pp. 187, 40 plates. An Archaeological Monograph published by the Società di Storia Patria per la Sicilia Orientale.

MYCENEAN POTTERY IN ITALY AND ADJACENT AREAS. By LORD WILLIAM TAYLOUR. pp. xx, 204, 17 plates. *Cambridge University Press*, 1958. 55s.

THE STRATEGY OF RESEARCH. By SIR GEORGE THOMSON. pp. 18. *University of Southampton*, 1957. 2s. 6d. In this, the Fourth Fawley Foundation Lecture, the Master of Corpus Christi College, Cambridge, discusses the factors which most favour research and quotes some of the wise sayings of his distinguished father, 'J.J.'

Forthcoming

DORSET BARROWS. By L. V. GRINSELL. pp. 160 (approx.), 4 plates, 7 maps and plans. To be published by the Dorset Natural History and Archaeological Society, with the aid of a grant from the Council for British Archaeology, by Longmans (Dorchester) Ltd., The Friary Press, Dorchester, Dorset. Price, including postage, 22s. 3d. (paper covers), 31s. 9d. (cloth); on orders received by 30 September, 1958, 17s. 9d. and 27s. 3d. respectively.

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